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THE AMERICAN SOCIETY OF NATURALISTS
PRESIDENTIAL ADDRESS[1]

ON several occasions during the last few years, indeed, ever since my election to the honorable position which I occupy to-night, I have been asked "What is the use of continuing the existence of the Society of Naturalists?" When one is in the full enjoyment of an honor so greatly appreciated as that which I now enjoy it is a veritable cold douche for some well-meaning but not altogether tactful friend to suggest that the honor may after all be an empty one and that the presidential chair I occupy is that of a society so moribund that it would be a kindness to let it turn its face to the wall and enter into its eternal rest. But a cold douche may have a highly salutary effect both in tempering a too great elation and in bringing one into the proper frame of mind for considering whether, after all, there may not be force in the suggestion. Gentlemen, I have passed through these experiences, I have considered calmly and, so far as possible, impartially the condition of the society and its relations to other organizations, and a reaction has set in. My appreciation of my position is reestablished and I am now more convinced than ever before that the Society of Naturalists has still an important part to play in the advancement of scientific achievement on this continent.

The society makes for the solidarity of those sciences which, in older days, were included in the term natural history. It was originated for the purpose of pro-

[1] Delivered on December 31, 1907.

moting that solidarity and its existence has been a struggle to maintain it against an increasing tendency toward segregation. Twenty years ago it was an organization of great vitality, including in its membership practically all the leading exponents of natural history in the eastern states, and its meetings were a stimulus and an inspiration to all who were privileged to attend them. But the very cause which called it into existence was destined in the course of a few years to sap its foundations. It was the outcome of the remarkable growth of interest in scientific education in this country which occurred in the eighties of last century and was associated with the establishment of two important departures in collegiate and university instruction.

One of these departures was the introduction into our college curricula of the course in general biology, inaugurated in England with so great success by Professor Huxley and fostered in this country by Huxley's one-time assistant and coadjutor, Professor Newell Martin, whose interest in the success of this society was both active and lasting. The establishment of this course of study, so philosophical in its conception, had the effect of disintegrating the older discipline of natural history, with certain results by no means in harmony with the ideals which the course was intended to realize. For its establishment led, in the first place, to a severance of geology from biology, a result not in itself to be deplored from the standpoint of efficient instruction, but, unfortunately, geology, as then understood, included both the dynamic and historical aspects of the subject and hence its separation from biology led also to the separation of paleontology. For the geologist paleontology is a means to an end but it is an essential constituent of biology. Further, the establishment of the course in general biology brought about an increased interest in zoology somewhat at the expense of botany, since the great majority of those in charge of the teaching of general biology were trained in zoological methods. This state of affairs undoubtedly acted detrimentally to the progress of botany, but we all rejoice to see that science so ably rising superior to her disadvantages and now coming again into her own.

Practically concurrent with the recognition of general biology as an undergraduate study was the development of graduate instruction as a proper and important part of the work of our larger universities, and as a result it became possible to supply the demand for teachers of this or that science with men thoroughly trained in modern methods and conversant with the literature of their specialty. For naturally graduate instruction tended toward specialization.

And this was the tendency that "like a worm i' the bud" fed on the "damask cheek" of the young society. The same interest in scientific education which led to the establishment of the society led also to increasing specialization, and solidarity gave place to segregation. The physiologists, as their numbers and influence increased, established a temple of their own where they might worship exclusively the goddess Function; the geologists, too, deserted the common shrine and fled to the mountains and valleys to erect in the groves altars to Pluto and Neptune; and the anatomists, ignoring the fact that their special cult was but a side issue of the broader worship of animal morphology, forsook the company of their fellows and wandered off to secluded spots where they might, without offence and free from disturbing suggestions from their coworkers, set up as an idol—a cadaver.

And so of the original membership there were left true to the parent society only the zoologists and the botanists, the latter at that time few in number, and the question

had to be faced whether the Society of Naturalists should become a strictly zoological assembly, should be allowed to lapse, or should continue to exist as a possible bond of union between the specialist societies. The *tertium quid* seemed the most satisfactory solution of the difficulty and the secession of the zoologists as the American Morphological Society gave opportunity for the reorganization of the Naturalists as a parent society beneath whose wing the several offspring might assemble yearly for mutual encouragement and fellowship. This new relation of the society of necessity curtailed its activities by reducing the time available for its sessions, but it still remained faithful to its original purpose, as it does to-day.

But additional factors came into the question. The rapid growth of the scientific spirit in the middle west which began in the later eighties and the nineties, one of the most striking features, it may be remarked in passing, in our educational history, called to that section of the country many enthusiastic naturalists who felt the need of maintaining just those wider interests which the society endeavored to promote. The society then had to determine whether it would extend its influence to this new territory and hold occasional meetings outside the pale of the northeastern states, but at the time it seemed that the limitations of the sessions to localities readily accessible to the majority of the members would better tend to conserve the energies of the society. It was recommended, however, that the naturalists of the central states should form a branch organization, which would do for that section of the country what the parent society did for the eastern territory, and this was done. But the growth of the scientific spirit in the more western section was not yet completed, nor is it even now. And with the increasing growth there arose a greater community of interests and more perfect intercourse between the two sections, leading eventually to the realization that occasional meetings of the one organization in the territory of the other, far from having a weakening influence, would further the objects for which both were striving. Hence the present arrangement, which, however, still requires modification in one respect, namely, in that of placing the Central Branch in the position to which its importance and influence entitle it—an equality with the parent organization.

A second factor of more recent development has been the irruption of the American Association for the Advancement of Science into the quiet and sociable serenity of convocation week, and the consequent desire on the part of some that the association should assume responsibility for all the fostering which the different scientific societies may require. Personally, I am not at all sure that the association as a mother by adoption can satisfactorily perform the functions of the real parent. In a family, real and adopted, so large and with such diverse interests, it seems almost certain that one or more unfortunate individuals may find themselves unable to secure the necessary shelter beneath the maternal wings and be forced to perch disconsolate upon the edge of the nest, "remote, unfriended, melancholy." A gradation of individualities is the rule in nature, and in our social combinations. Between the organ and the person there is an intervening individuality and it is that individuality which is lacking in the organization of the association, but which is represented by this society. The solidarity, which is the *fons et origo* of the Naturalists is, I am well aware, the aim also of those who desire absorption into the association, but under the present organization of that body the solidarity of the biological sciences would, by the absorption, be lost,

and they would become, if I may be permitted to misquote a celebrated definition, members of an indefinite, incoherent heterogeneity, instead of, as now, parts of a definite, coherent homogeneity.

I have thus briefly sketched the history of the Society of Naturalists with the object of showing those among us who may not be familiar with its past, how steadfastly it has clung to its original purpose through all the crises which have threatened its existence. Is not the idea for which the society stands worthy of such consistency? And are there not questions pressing in upon us to-day which stand in need of consideration by the united strength of the society?

These interrogations have been answered in part by the discussion of this afternoon. The undertakings of biologists are becoming broader year by year and are more and more demanding cooperation for their successful completion. The time-honored discipline of natural history has been divided into numerous specialties, each of which is as wide as the whole field of natural history as our fathers and grandfathers knew it. Encyclopedists were possible in their days, although even then it required an exceptional ability to be a master of the entire field. But encyclopedism died in this country with such men as Louis Agassiz, Leidy and Cope, and we of to-day find our capabilities fully tested in mastering one small division of the older discipline. We may comfort ourselves somewhat with the thought that the limitations of to-day are not due so much to differences in the men as to differences in the scope of the subjects. The lakes of our predecessors have broadened to seas and the seas to oceans whose farther shores are far beyond the limits of any one man's horizon, and hence specialization has become a necessity, and where but a few years ago we had zoologists, we now have systematists, anatomists, embryologists, cytologists, experimentalists, statisticians and ecologists. But let me quote the words of one of our distinguished members: "*Union* is just as essential a part of the law of progress as division. If specialization is a necessity, so is organization. But there is this difference between the tendencies—that the one precedes the other and comes into recognition first. Specialization has already forced its way to the front, and is nearly everywhere recognized as a necessity; organization follows, but lags lamentably behind the needs of the times." Throughout the organic world we see continually contrasting forces combining to produce progress. We have variation and heredity, division of labor and organization. Specialization is with us, and the Society of Naturalists is but striving to add the other factor which makes for progress —cooperation.

The necessity for cooperation in scientific research is no new need evoked by the increasing specialization of the times. Even in the days of Lord Bacon it was presented as a desirable ideal, and nowhere can we find a more definite advocacy of its employment in the investigation and application of scientific problems than in the plan set forth in the *New Atlantis* of the duties of the fellows of Solomon's house. Such a complete plan is, however, impracticable so long as human nature remains as it is. We would all be "interpreters of nature" or at least "lamps." But to function thus we must needs cooperate with our fellows, we must meet together to tell of our investigations, to learn of those of others and to take counsel with our coworkers as to the further elaboration of our results. And it is this form of cooperation that the Society of Naturalists promotes. If the society did nothing further than to bring us all together on occasions such as the present its existence would be fully justified.

But can not its influence be enlarged? Are there not problems bearing upon the advance of the biological sciences which require the cooperative action of just such a body as this for their solution? We have listened to-day to suggestions concerning cooperation in problems of investigation and of these I need not speak further. But there is another field in which, I believe, the society, by its influence, can accomplish much that will, both directly and indirectly, aid in the advancement of the biological sciences. In the early days of its existence this society took great interest in the question of scientific instruction in our schools and colleges, and I recall especially two reports submitted to the society on this question, one by Professor William North Rice and the other by Professor S. F. Clarke, which contained much that was of value and undoubtedly of influence in shaping the scientific course in many of our schools and colleges. This was many years ago, and now, with the increased interest which obtains in scientific instruction and after the numerous discussions and reports on the place of scientific studies in our secondary schools, it would seem that the time is again propitious for a pronouncement upon the subject from such an organization as this.

Partly from lack of time, but more especially from lack of the necessary information, I can not venture to discuss all the phases of this question. But do we as a body of working biologists properly understand the conditions of science-teaching in the schools, and have we shown sufficient interest in bringing it to that state of efficiency which its importance demands? In later years a wave of nature study has passed over our primary schools, driven by Froebelian breezes. But, unfortunately, in many schools it seems that the Froebelianism which should blow as a gentle zephyr has been permitted to increase to a hurricane and the wave of science study, instead of being an educational blessing, has carried devastation on its crest. Two of our members, Professors Hodge and Bigelow, have accomplished much by their endeavors to establish nature study upon a proper basis and their work deserves a greater meed of credit than it has hitherto received. But even yet, so far as my observation and information extend, the teaching of nature study is in many schools in the hands of inefficient instructors, untrained in the methods and purposes of such instruction, and the result is a minute crumb of solid food overlaid by a heavy coating of mawkish sentimentality. The principal aim of nature study should be to train the child to the observation of natural objects and phenomena and to awaken in his mind a healthy curiosity as to their meaning and significance. In other words, its purpose should be to develop in the child the scientific spirit, which is not inborn but requires development. Its primary object should not be a directly utilitarian one and it should certainly not be used as a means of evoking an unhealthy and unnatural sentimentalism when no sentimentalism should exist. Surely in a search for the sentimental nature is the last place to which we should turn. Perhaps the causes of the mistakes in nature study are largely due to conditions which are beyond our control, but have we done our duty in upholding the hands of our fellows who are striving for efficient instruction, in calling the attention of those in authority to errors in method, and in endeavoring to set science teaching in the primary schools upon a proper basis?

We are accustomed to regard the German system of scientific instruction as very efficient and yet it is noteworthy that a joint committee for the German Zoological Society and the Gesellschaft für Naturforschenden Freunde und Aertzte is now

at work upon the question as to how science teaching in the schools may be brought to a proper degree of efficiency. If a need for improvement is felt in Germany, is it not likely that it is present also with us? How often have we heard a colleague in one or another of the scientific departments state that he would sooner receive into his classes students entirely unfamiliar with his subject, than those who had received some training in it in the high school? An occasional statement of this sort might be attributed to that indiosyncrasy which is popularly regarded as a characteristic of a university professor. But it is made too frequently to be altogether due to that cause, and allowing for a certain amount of rhetorical exaggeration, the statement is an indication that a need for improvement in science teaching in our high schools certainly exists. From what body could the initiation of a movement for the improvement of instruction in the biological sciences more appropriately come than from this society? In passing, let me recall that at our last meeting in this city we had the pleasure and profit of listening to an eloquent and serious arraignment by our then president, Professor Sedgwick, of the prostitution of scientific teaching to the intemperate propagandism of a powerful organization. Have we as a body or individually followed up that deserved indictment as we should? The recent publication in SCIENCE of a letter from the organization in question shows that the snake is not even scotched and that with cool effrontery the organization proposes to continue its dictation of what text-books of physiology shall be used in our public schools.

And even in connection with our college courses, in which the majority of us are more directly interested, there is opportunity for this society to exert a healthful influence. Our college education is at present in a stage of transition, and it is difficult at the moment to determine what will be the final outcome. But in one direction at least there seems to be a definite tendency and that is toward a more distinct cleavage between undergraduate and post-graduate work. It is showing itself in our professional schools, which are more and more approaching the ideal condition in which they will represent post-graduate courses, students being allowed to enter upon the special work of the schools only after they have laid a broad foundation for their professional studies by completing a collegiate course. In other words, the professional schools are beginning to recognize the value of a broad training as a preparation for successful specialization. This movement should receive a hearty support from this society, for it is at one with its aims and it is a movement for whose further expansion there is still ample space. Up to a few years before the organization of this society the completion of the literary curriculum meant the completion of one's education; the man who was entitled to write A.B. or some such letters after his name was the final product of our educational system. True, there were higher degrees, A.M. and what not, but the training for these was more or less perfunctory and unorganized, and there were also occasional students who had the opportunity to carry on their studies beyond the ordinary four years of the university curriculum. The majority of these, however, found it to their advantage to pursue their later studies in the old world universities, and especially in Germany, which first had recognized the advantage of making the university something more than the mere dispenser of knowledge already acquired. In 1876 the cleavage between undergraduate and post-graduate studies—similar to that between the German university and gymnasium—became established in this

country, and now there are few of our large universities which do not recognize it in giving prominence to university work in what are called graduate schools.

But in one respect the conditions in our graduate schools are very different from those obtaining in the German universities, for the instructors in our graduate schools are also, almost without exception, teachers in the undergradute or collegiate department and are comparable, as some one has pointed out, to a Gymnasiallehrer who also lectures in the university. And in this lies a serious defect, for it has led to the encroachment upon the collegiate course of studies which properly belong to the university or graduate course. The enthusiastic teacher who is also an investigator finds his greatest pleasure in leading his students on toward investigation and he is too often inclined to carry them with him into that kind of work before they have received a foundation of sufficient breadth and solidity to make such a course advisable. And the adoption of an extensive system of electives in the collegiate course has favored the development in this way of precocious investigators who so frequently are like the seeds which fell in places where they had not much earth and forthwith sprang up, "and when the sun was up they were scorched; and because they had no root they withered away."

I have recently been looking over a number of college calendars with a view to ascertaining the extent to which specialization might be carried by undergraduates. And let me say in passing that as a result of the examination of the calendars my opinion as to the intellectual capabilities of the American undergraduate has been greatly increased. The young man who can successfully thread his way among the multitudinous courses with their limitations and continuations as stated in the larger calendars, and from these select as consistent and suitable a course as the majority do, manifests a degree of intelligence and perspicacity which augurs well for the race.

It would be both unprofitable and tedious to give you the complete results of my studies in this direction, but I may briefly indicate what I found to be the case in regard to specialization in one subject, namely zoology, in three or four of our leading colleges. The different standards employed in estimating the credit value of a course renders an exact comparison of several colleges somewhat difficult, but so far as I can understand the schedules presented the results are as follows: In four of our most influential universities I find that a student out of the total number of scheduled hours may elect in zoology in A, 33 per cent.; in B, 41 per cent.; in C, 45.5 per cent., and in D, 68.3 per cent. This represents undergraduate work only and the enormous inequality of the courses in the different institutions is most striking, D university, for example, allowing over twice as much specialization in zoology as A. This difference is necessarily associated with great differences in the amount of time devoted to the humanities or non-scientific studies, and these also stand in relation to the amount of specialization in scientific studies as a whole, which the various curricula permit. Thus in A a student in zoology may take an additional 30 per cent. of his studies in other sciences, making a total of 63 per cent. of scientific studies; in B he may take 44.3 per cent. additional in science, or a total of 86 per cent.; in C an additional 44 per cent., or a total of 89.5 per cent.; and in D 26.7 per cent., or a total of 95 per cent. Or, to state the reverse of the story, a student in A may secure his A.B. degree only after taking 37 per cent. of his work in non-scientific studies; a student in B may graduate with 14 per cent. of his studies in non-

scientific subjects; a student in C with 10.5 per cent. of non-scientific subjects; and a student in D with only 5 per cent. It seems certain that one or other of the extremes must be utterly bad in principle. The one represents a broad collegiate training upon which the student may build a specialized university course, the other is extreme specialization by which the student is carried into the graduate type of work before he has acquired a well-rounded collegiate training. Indeed, it may be pointed out that in the extremely specialized course the student is allowed to devote ten hours a week throughout his final year to research in zoology.

I do not wish it to be understood that such narrow courses as have been mentioned above are frequently taken. Indeed, I believe that the great majority of students of their own accord choose rather a broader course, and, furthermore, in some institutions elections must be approved by a member of the faculty or by a committee, a system conducing to some extent to the prevention of extreme specialization. But such courses as I have indicated are possible; indeed, in the institution D a student who expects to teach zoology is openly advised to elect as many as possible of the courses offered in that subject, that is to say, to elect the extremely specialized course mentioned above. Surely such advice betrays a sad misunderstanding of the proper functions of the college and university and must tend in the long run to prejudice rather than to advance the claims of zoology to a place among the so-called culture studies.

Nor do I wish to imply that zoology is the only grievous offender in this respect. Results similar to those already given may be obtained from the study of possible elections in other courses, both scientific and non-scientific. A student may graduate from college without ever having seen the inside of a laboratory or listened to a single course of lectures on a scientific subject during his four years of attendance. And to give such a student an imprimatur which should imply that he has received a broad collegiate foundation is a crime against good scholarship.

But this is not the occasion for a general discussion of this question; we are concerned with it especially as it relates to biology. I may say, I believe, without an imputation of Chauvinism, that biological investigation on this continent stands second in quality to none, and it should be our endeavor to see that quality is not sacrificed to quantity. A multitude of effusions characterized by narrow specialism will advance the position of biological research far less effectually than a more moderate product in which thoroughness is combined with a scholarly appreciation of the scope of the problems in hand. This latter desirable conjunction will not be secured by devoting a considerable part of a student's collegiate course to university studies. An instructor in collegiate courses may and should, by both precept and example, set forth the methods of the investigator and endeavor to awaken in his students the spirit of the investigator. But let him see that a vaulting ambition is not allowed to o'erleap itself, and secure for his students that broad outlook which alone can produce the scholarly investigator. That extreme specialization should be even possible in an undergraduate course is a serious mistake. It narrows the field of vision and is a serious obstacle to the carrying out of the cooperation so much needed in biology. And cooperation implies solidarity, the main plank of the platform upon which the Society of Naturalists stands. Surely there is still work for the society both in advocating a system of training in our schools and colleges which will make cooperation in investigation pos-

sible, and in promoting the solidarity among biologists which will make cooperation feasible.

J. PLAYFAIR MCMURRICH

COOPERATION IN BIOLOGICAL RESEARCH[1]

THE idea of cooperation in science is not new; our universities, learned societies and publications represent or involve forms of cooperation that are well established and have demonstrated their usefulness in the progress of science. Without them, progress would be painfully slow. They are, in fact, the very framework and supporting skeleton of science, without which there might be life indeed, but at most aimless amœboid movement, no dignified or effective progress.

I suppose it was not intended that the present discussion should concern itself with such old established organizations, but rather that it should deal with needs that have arisen as a result of recent growth of science and its increasing specialization, and which are not adequately met. Organization must keep pace with specialization, if the true objects of specializing are to be attained.

The last decade has witnessed the origin or farther development of institutions planned to meet the specific needs of the present, and organized to anticipate the growing demands of the future. I name various departments of the national government, the Carnegie Institution of Washington, the Wistar Institute of Anatomy and Biology, the Rockefeller Institute for Medical Research and the McCormick Institute for the Study of Infectious Diseases. These institutions recognize the fundamental importance of research for the well-being, nay, for the very life, of the commonwealth, and they also recognize cooperation as the vital principle in the conduct of research. The institution that inbreeds, that does not seek for the original and productive investigator, and that does not lend its own cooperation and secure his is on the high road to ineffectiveness.

I believe, however, that the full conception of cooperation in scientific research is not usually grasped and that the logical outcome of the principle is, therefore, not really understood: An organization may be formed that proposes to make cooperation with scientific men and institutions its main business; it may propose to seek out the original investigator wherever he may be found and to support his work in every possible way; it may welcome every new branch of scientific investigation and propose to favor it according to its importance and its needs; and yet such an institution may not be fully cooperative. It may be privately controlled; if so, its impulses are primarily benevolent and not free, guided by tradition and charter and not by the native interests of the governing body, and for these reasons apt to fail to profit to the fullest extent by the fertilizing influences of new conceptions.

The fundamental idea of cooperative organization is a free association of individuals that proposes definite ends and effects an organization to attain them. The members of the organization are at the same time the court of last resort; they may elect representatives as a board of management, or as officers of the organization; but the representatives are responsible to the organization for the conduct of affairs. The functions of such an organization are not benevolent, but free, for the members are vitally interested in the conduct of its affairs and they are themselves the governing body. The organization is plastic, responding to new ideas, so long as membership in it is determined by broad prin-

[1] Discussion before the American Society of Naturalists, December 31, 1907.

ciples and not merely by personal considerations.

Our scientific societies are the type of such organizations in science. The members make their own laws, elect new members, appoint officers in rotation from the membership, read their own papers and pay their own expenses. In turn, groups of members take on the duties of hosts to the others, and it is a genuine and deep pleasure, for are they not returning the numerous hospitalities that they have received? Such meetings are among the best events of the year in science, the most stimulating, the most fraternal; and the impetus to the progress of science is incalculable.

The Marine Biological Laboratory is another example of a free cooperative organization in biology. I believe I need not apologize for using this organization to illustrate my remaining remarks, because it belongs to the Naturalists of America and it illustrates better than any other institution with which I am thoroughly familiar certain working principles of cooperation in biology.

The government of the laboratory is vested in a corporation consisting of 64 life members and about 125 annual members, and a board of trustees of 31 members (at present, full number 35), of whom three are *ex officio*. The corporation meets annually to elect a clerk, treasurer and eight trustees to serve four years. Membership in the corporation is by election by the board of trustees. To the latter body is delegated the administration of laboratory affairs, and they appoint all officers and agents. Of the 189 members of the corporation, 129 are professional biologists; of the 31 members of the board of trustees, 25 are of the same class. The corporation has the power to modify the policy of the laboratory in any respect by virtue of its control of the membership of the board of trustees.

The government and ownership of the laboratory thus rest absolutely in the hands of American biologists, and this is the first and fundamental cooperative feature of the organization, viz., cooperation of those interested in affecting an organization for research. I believe that much of the usefulness and effectiveness of the institution rests on this basis. It establishes a thorough democracy of sentiment, which is more nearly like the sentiment of our scientific societies than that of any other institution that I know. Out of this grows a feeling of loyalty that is shared in greater or less degree by nearly all who have worked at the institution. These now represent practically every university and college of the country, most of which are represented either in the board of trustees or in the corporation.

This fundamental cooperative principle was established at the foundation of the laboratory, though the proportion of professional biologists in the membership, both of the corporation and of the board of trustees, was less then than now.

A second important form of cooperation exists between the laboratory and other institutions. Many of our institutions require the facilities of a marine station for the research work of their biological departments. Some have sought to supply these facilities independently, but I believe I am correct in saying that this method has not usually proved permanently successful, partly because of the expense of the undertaking, and partly because such an individual undertaking lacks the inspiration that comes from the presence of representatives of other institutions, who bring in new ideas and establish a congenial community.

In the year 1891, after four years of ex-

perience in conducting the affairs of the laboratory, a committee of the board of trustees prepared and sent out a circular letter requesting the cooperation of the biological departments of our colleges and universities in the maintenance of the Marine Biological Laboratory. The form of cooperation suggested was that of subscription for students' tables or research rooms, the former at $50 each and the latter at $100 each for the season. In this way the laboratory anticipated the need of biological departments for marine facilities both in instruction and in research.

The response was unexpectedly general and prompt; the following institutions subscribed at once: Columbia, Brown, The Missouri Botanical Garden, Williams, Chicago, Massachusetts Institute of Technology, Rochester, Bryn Mawr, Mount Holyoke, Vassar, Wellesley, Cincinnati, Miami and Northwestern. The majority of these institutions have continued their subscriptions up to the present time. Others have since come in, some sporadically, others continuously. Last year the number of subscribing institutions was 17. There has not, however, been considerable growth in this respect; and this is perhaps partly due to the fact that the laboratory has rarely refused a free working place to competent applicants, unless space was lacking.

It is clear that an institution may secure for its investigators by this form of cooperation the best of facilities for marine work at a cost many times less than would be required on an independent basis, with the added advantage of association with representative investigators from other laboratories. At the same time such an institution is aiding to support an organization that supplies one of the most general needs of American biology.

Special forms of cooperation with institutions are entered into from time to time. Relations with the Woods Holl Station of the U. S. Bureau of Fisheries have always been mutually helpful. The laboratory also exchanges investigators' tables with the biological stations of Canada and desires to enter into similar relations with other marine laboratories. For three years the laboratory furnished working places for twenty appointees of the Carnegie Institution and this relation was a great aid to the Marine Biological Laboratory at a critical juncture of affairs, and furnished a permanent stimulus and incentive to the prosecution of its work.

It is very desirable that such cooperative relations with other institutions should be extended and strengthened; and it is probable that this will take place in the near future. The maintenance of such relations implies that they shall be mutually advantageous. I believe that this has been demonstrated, and that, in proportion as this is realized and the spirit of research increases in our institutions, such cooperative relations are bound to grow. The principle of cooperation does not mean that all shall do the same amount, but that all shall enter into it in the same spirit and do according to their means and opportunities.

A third fundamental form of cooperation is that of and with the various subdivisions of biological inquiry. Four departments are formally organized in the Marine Biological Laboratory; in the order of their establishment they are: zoology, botany, embryology and physiology. These are, of course, broad divisions and their organization as departments does not mean that other subjects are excluded; indeed, the laboratory welcomes any biologist with a problem in the solution of which the facilities or fellowship of the organization may aid. There has been a good deal of

research done both in pathology and in psychology at the laboratory, and it is hoped that it will grow; the laboratory is ready when the time is ripe to organize departments in these subjects.

The problems in the various departments of biological research are so intricately interrelated that each department is sure to be of aid to others in many ways often curious and unexpected. This is not a matter for surprise. It only emphasizes the necessity of the broadest organization of our work if any subject is to march forward with the least degree of impediment.

Indeed, I question very much if the work can logically be confined to the subject-matter of biology. Our physiological chemists are already urging a chemical laboratory. This seems necessary for the physiologist who has chemical problems, and it may prove attractive to the chemist who has biological problems. A department of chemistry in connection with the Marine Biological Laboratory would be of distinct advantage to every other department; even the morphologist has his chemical problems, particularly in connection with the complex matter of stains.

An institution organized in the manner outlined is in a position to develop in harmony with its environment, and should be in little danger of outliving its usefulness, because it is national, representative and plastic. It should thus represent at any time the best traditions and methods of research so long as it maintains the fundamental forms of cooperation: its free organization, its free cooperation with scientific societies and men, and the free cooperation of the biological sciences within the common organization. If these are maintained, it must inevitably keep pace with the increasing subdivision of biological specialties and be guided by the community of ideas and problems in different fields.

An important result of the cooperation of biologists in the maintenance of the Marine Biological Laboratory is the direction given to research. It is true, of course, that every productive investigator aids in giving direction to research; he has students and he has imitators. Where a number of such investigators come together for considerable periods of time and problems are discussed, there tend to be a clearing of ideas and sharper definition of problems. This effects a larger circle, and the influence spreads. The best results of this sort can be attained only if people discuss their work freely, and do not keep it locked up until after publication. I believe that a considerable effect has been thus produced in direction of research, particularly in cytology, experimental embryology and physiology at the Marine Biological Laboratory. Organized efforts to direct the research of others, excepting one's own students, are apt to be futile, especially on a large scale, and in a large subject such as biology, where fundamental problems are dimly perceived or perhaps yet unsuspected. For this reason and others even more important, the institution has never prescribed or limited subjects of research.

I have spoken thus for the most part about the work of the Marine Biological Laboratory, because it illustrates in a practical working model various possible forms of cooperation; and because such a demonstration is worth much more than a merely academic exposition of the nature and advantages of cooperation.

Frank R. Lillie

Especially when only a few minutes can be devoted to a very large subject, definition of terms is of much importance.

Cooperation in education or research may be compared with efforts as dissimilar as those of an army, a swarm of bees, an anthill after enumerating all of its denizens, or the cells or cell-complexes of that unit that we biologists know as an individual animal or plant.

Every professor having to do with graduate instruction exemplifies a simple type of cooperative alliance in his relations with students: sometimes stating a problem, encouraging them with a nod of appreciation, or setting them to thinking by a suggestive question in the Sprechstunde to which association with them is limited, while they are left to fight out their own salvation at other times; sometimes being brain and ganglion to their muscle, and himself doing all but the mechanical parts of their thesis majors; occasionally, perhaps, drawing equally facts, inspiration and reputation from their surpassing initiative, energy and success—and then possibly being even more than an incubus.

There may be good administrative reasons why a research department should not show seeming narrowness of vision and purpose; but there is a great chance, in a department blessed with armies of capable graduates, to distribute between them the details of a broad study, the blocking out and accomplishment of which marks its architect as a master in the truest sense.

May I suggest that cooperation—the mention of which instinctively sets us to thinking of enlisting for our own purposes the effort of remote workers—may sometimes at least, like charity, begin at home; and that many of the good theses which now appear to the average critic as dissociated from one another, and without obvious environmental relation, can be given thus an excellent ecological meaning?

People with a capacity for business organization see that this simple type of cooperation might reach much farther if the various graduate schools of biology were further to differentiate and coordinate their respective effort. It is safe to say that the students of a given department, in which they stay for only a few years, can effect such a coordinated and cooperative attack on the several parts of a large and divisible problem only by chance or a miracle; but the result is quite within the power of the permanent heads of departments, if they are willing to take it up and desire to do so.

Popular interest in biology to-day centers about the plant or animal as a mechanism, the plant or animal in action, rather than as illustrating that abstract concept called, sometimes sarcastically and usually vaguely, a species. Illustrations of intradepartmental cooperation are afforded by a few of the great morphological and physiological laboratories, and in the studies which Bateson is having made in genetics and Pearson in biometrics. Who can doubt that we who admire the great men who edit *Pringsheim's Jahrbücher* are really able to characterize its editors, Strasburger and Pfeffer, as leaders in their profession almost as much because of the correlated contributory studies of their pupils as from their own great investigations?

For interdepartmental cooperation, which I understand is admirably exemplified in current astronomical work, illustrations in our own field may be taken from the now unpopular—but fundamentally indispensable—systematic branch of botany. The master mind in this field to-day, Engler, is exemplifying in a large way, by the publication of "Das Pflanzenreich," what united effort may accomplish; and our own incipient "North American Flora," under Britton's editorship, has been launched on the same lines—which have long been successfully followed in the much simpler preparation of encyclopedic matter.

There appeared at one time a possibility that Mr. Carnegie's great research foundation might knit together and unite the workers of our country into a net by which the depths of science should be dragged, but there is a bottom to every purse, and, large as its resources are, the Carnegie Institution has found the requirements of investigators to be still larger, and its policy seems to be crystallizing wisely into ample support of relatively few definitely controlled studies rather than a broadcast dissipation of its resources without such control. There is perhaps nowhere a better illustration of efficient, self-centered organization on a large working basis, but our national Geological Survey offers an equally good illustration of this type.

When the agricultural experiment stations were established, with national support and under the supervision of a national bureau, some persons thought that cooperation between the stations might be secured through the latter. Whatever the purpose of the law may have been, as conceived, its provisions, as embodied in legislation, have given to the central office little authority beyond financial supervision. Perhaps it is best that this should be so—every question is many-sided: but it is generally conceded theoretically that larger strides might have been made in agricultural science by greater concentration and correlation of the effort of the stations. The newer addition made to their equipment for investigation by the Adams act more clearly provides for this, and promises adequate results; but it is already bringing prominently to the front other cooperative needs, the most important of these referring to the channels of scientific publication.

As a matter of fact, the Carnegie Institution and United States Geological Survey do not illustrate cooperation in the sense in which I understand the word to be used this afternoon. Each is really an aggregation of workers whose tenure of office as well as their scientific activities are more or less definitely under the control of a recognized chief. Their effort, because of this control, is as certain to be productive of desired results, under good leadership, as is that in a well-managed factory—subject always to the greater difficulty of directing the activity of educated men with wishes of their own as to the application of their talent. The—not always popular and variously successful—efforts of such an organization to enlist extraneous volunteer cooperation are beset by peculiar difficulties that are not to be disposed of in a word; one of the greatest of these perhaps lying in unexpressed and unaccepted but no less real punitive power at the central desk. The ultimate coordination of experiment station activities, if effected, can only add another illustration of good administration of a self-contained organization which pays its constituents for the effort that it therefore controls.

Real cooperation, though it will always have to direct its aims with reference to those of such powerful endowed aggregates as may exist, can hardly be looked for through the latter. Its units must be the scattered men of science who constitute the university and museum forces of the country—one or a few in a place. The difficulties of effecting and maintaining such cooperation are identical in part with those underlying good government, and can be met, apparently, only in the way in which municipal problems are met successfully.

Is such cooperation desired? The native Mexican never hails the coming of the civilizer after he has once understood him and his ways; more work and greater competition are what he sees as his own portion. His question, Is greater progress worth its cost? is worthy of consideration. If it be granted that it is, it is hardly

necessary to await the coming of a leader in order to effect organization through which it may be reached. The cry for a king is answered, in the proverb of our profession, by misfortune. Our tastes and institutions are democratic. Our greatest achievement of cooperative scientific organization promises to come through the methods that we know and like. Initiative lies at the door of a few universities whose graduate departments are the home of the larger part of the biological research of the day. We really can not be so poor in men as not to be able to find an executive of ability and tact, if we desire him and search for him. Not impossibly, when found, he may prove to be so conscientious in "pottering" over his new task that his own hand will lose its cunning in technique, and opportunity for mental concentration in his own chosen field may be sacrificed to the new duty. If so, and the duty be performed, need we begrudge him the recognition that, if successful, he must win as the coordinator of our research?

The decision to form an effective research organization must be made by us if it is to be made; action on such a decision is equally ours; responsibility for cooperative success must depend in equal measure on investigators and executive. There is no strong reason to doubt that such success is attainable; but the purest spirit of democratic government, dominated by that love of advanced scholarship which makes and marks the investigator, seems essential for its permanence.

WILLIAM TRELEASE

FROM the history of biology, it would be easy to show that the idea of cooperation had not been always with us. Indeed, so late as the founding of the Naples Station, when Dohrn sought the approval and support of the venerable Eulenberg, that worthy refused to aid him, on the ground that Dohrn's plan would exhaust all zoological problems within twenty years.

Eulenberg's fears have proved unwarranted, and we no longer regard the supply of problems as dangerously limited; in fact, it is the very opposite condition that is most in evidence. The attempt has been made to meet this superabundance of opportunities by an increasing division of labor, and it is pleasant to note that the workings of specialization in the field of biology, impress us with the fact that specialization and cooperation are but two aspects of the same process.

This idea is so familiar, however, that I do not need to expand it, yet despite the general acceptance of the broad fact, the intimate nature of the relation between cooperation and specialization is often rather vaguely felt, and the present discussion should assist us to intensify our consciousness of this intimacy, and so make clearer how we may, and ought to act.

In its immediate and simple form, cooperation hardly requires to be discussed. We are familiar with such examples of cooperative work as Keibel's "Normentafeln," or the biological investigations undertaken in behalf of the alcohol commission. Matters like these arrange themselves. On the other hand, even without previous agreement, we get similar and in some ways better results, when a number of investigators independently direct their attention to the same problem, as has occurred in the study of Mendelian inheritance, or of the auto-regeneration of nerve fibers.

In attempting combinations for the solution of large problems it must be kept in mind that any arrangement which suppresses or eliminates the pleasures and excitements of the hunt for truth, or which cramps the cooperator, is in so far faulty. Against this we must be always on guard, for it is agreed, I believe, that the solution

of problems and the answering of questions is of most worth as a means for developing the workers themselves, and good plans for cooperation must be in harmony with this idea.

Recognizing these limitations, by which cooperative endeavors are necessarily restricted, and at the same time recognizing the existence of relations which might be improved by a more vivid appreciation of the cooperative spirit which underlies biological activity, my colleague, Dr. Greenman, with the assistance of his advisers, has undertaken to utilize the Wistar Institute for the purpose of such improvement.

Permit me to mention two things which have been done, and which are distinctly promising. They constitute my special contribution at this time.

On looking over the field a year ago, it appeared that American biologists lacked suitable facilities for the publication of papers which were extensive and required ample illustrations. Most journals did not feel justified in devoting to such long single communications the space and money which are demanded for them. Nevertheless, papers of this character mark a step in advance, for they result from the effort to be more critical and thorough, and plainly they deserve encouragement. It appeared, therefore, that we could advantageously cooperate with investigators in this country, by arranging for the publication of such researches, and through the appreciative generosity of a colleague it has been possible to do this by reviving the *Journal of Morphology* under most favorable conditions.

In your presence it is hardly necessary to enlarge on the significance of this step, but I have ventured to call attention to it, as one example of cooperative activity.

My second example is from quite a different field and relates to the collection and preservation of material, in order to make it accessible to investigators at large. The endeavor to do this has grown out of a movement started and fostered by His. Some years ago His persuaded the Associated Academies to appoint commissions for embryology and for brain study, in order to develop cooperative work in both these fields. His knew from personal experience that studies in these departments had reached such a stage of elaboration, and were so largely comparative, that the labor of preparing the material for a given research often overtaxed the powers of even the most industrious, and the hour had come to assist investigators by gathering and storing at central stations series of sections and related material, which should be available for all.

This idea naturally appealed to us, and we are now endeavoring to develop it, laying emphasis at present on the material which illustrates the structure of the nervous system. The plan gives to the museum of the institute something of the functions of a library, with this difference, that it handles specimens instead of books.

Such specimens are derived from two sources: from the laboratories of the institute itself, and from elsewhere. In connection with the latter source, permit a passing comment.

We all have occasion to lament the fact that many of our promising scientific youths, after an encouraging start, turn aside from pure science to follow the primrose path of some more remunerative calling. For their loss and ours, we must always grieve, but there is comfort in the thought that in a measure it may be possible to save the pieces. The youth is gone, his special skill diverted, but his preparations may still be rescued for the benefit of others.

Too often these preparations find their last resting place in some forgotten trunk

or lofty laboratory shelf, from which they are never recovered. Such loss should be prevented, and at this point the museum comes forward and offers to care properly for these materials.

To do this effectively, however, there must be genuine cooperation on the part of the investigators. Such material is not worth storage or arrangement unless accompanied by descriptive notes, notes so complete that by the aid of them the material may become really useful to a second person. The museum can offer the opportunity, but the acceptance of this by the investigator implies also the obligation we have named, and it will be of interest for us, in the course of years, to observe how far the obligation will be met. Such is my second example, and it completes the instances which I desired to present.

Before closing, however, allow me to reenforce the general statement that aside from the obvious occasions for simple cooperation, which in a measure take care of themselves, there also exist between investigators more complex relations which offer special opportunities for cooperative treatment. The examples given have been selected as illustrations of such opportunities, and indicate how some of these may be utilized not only for the general advancement of biological research, but also for the stimulation and assistance of the individual biologist.

HENRY H. DONALDSON

AGITATION in favor of cooperation and coordination in scientific research has been noticeable in recent years in many departments of science. It seems quite possible that the importance of cooperation and combination of effort has been borne in upon us by recent demonstrations of the effectiveness of such movements in industrial enterprises. However this may be, it is safe to assert that the general idea has come to the front repeatedly of late years, and it is probable that as an outcome of the discussions aroused some definite attempts will be made to utilize this principle more fully than has been done heretofore in the advancement of science. Indications of such an effort may be seen in astronomy in the organization effected for the preparation of an astrographic chart, and in the successful establishment of the International Union for Cooperation in Solar Research. As a matter of history, we are all aware that the idea is not a new one in science. This fact has been especially emphasized by Merz in his valuable book upon "The History of European Thought in the Nineteenth Century." He states that cooperation formed the underlying principle upon which the great academies and scientific societies of Europe were based. In the organization of the Académie des Sciences, particularly, the idea was kept clearly in mind, and, indeed, was most successfully applied in various important pieces of work, such as the measurement of arcs of the meridian and the determination of the variations of gravity in different latitudes. We may believe, in fact, that the recognition of the value to be derived from combined effort on the part of those interested in a common pursuit explains the existence of such societies as this which we are attending to-day. No one probably is disposed to doubt the importance of cooperation when the term is interpreted in a broad way, but if we give it a narrower connotation in the sense of an intensive combination of the scattered energies of many workers there is, perhaps, room for differences of opinion in regard to its value. The point for discussion, as it presents itself to my mind, is whether or not it is desirable and feasible in the actual work of investigation to seek for an intelligent coordination of the activities of numerous individuals, and to attempt to

focus this combined effort upon specific problems. Speaking in general terms, it is evident that the bulk of the investigation going on at present is not being conducted on this principle. The productive investigators in the various laboratories of the world are working independently. The problems that engage their attention are determined by personal interests or accidents of equipment or opportunity, and their researches are not correlated except in so far as certain problems come to the front from time to time, and by the general interest which they excite attract for a period numerous workers to a common line of study. The fortunate investigator who unearths a new idea, or devises a new method of importance, is sure to have many followers, and there results for a while a certain kind of cooperation, which is lacking, however, in the element of intelligent coordination; so that oftentimes there is an apparent waste of energy and material, due to the fact that the individual investigator is unable or unwilling to make full use of the results obtained by his coworkers. The method of independent investigations needs no apology or defense, and we should be careful not to minimize its importance. The competition that it implies encourages originality and carries with it all the benefits that accrue from differences in point of view. It is to this kind of investigation that we must look for our epoch-making discoveries, so at least we may infer from the past history of science. No one surely has any wish other than to see this kind of research grow in volume and importance in this country. Nevertheless we may ask whether it is not possible that in some ways better and quicker results would be obtained by directed cooperation. In the accumulation of reliable data, for example, by the concentrated application of approved methods of work. It needs no argument, I am sure, to convince any experienced worker in science that eventually such accumulated knowledge will cause of itself the destruction of false theories and the development of newer and truer points of view. In my own subject, at least, it is undoubtedly a fact that brilliant discoveries have come, as a rule, not as a bolt from the blue, but from a slow accumulation of diverse facts and theories which, eventually, in the mind of some one gifted worker, when the time was ripe, have burst forth as a new conception. Our individual workers of genius must be supplied with raw material in the way of facts and theories in order that their talent may be productive of real good, and it is in the accumulation of this raw material that most of us make our contributions to the advancement of science. It is in this direction also, as well as in the utilitarian application of scientific knowledge, that cooperative work, as defined above, might be depended upon to greatly accelerate the rate of progress. From the point of view here adopted the success of cooperation in scientific investigation must depend chiefly upon the possibility of devising an efficient organization for carrying it on, and obviously two essential requirements of such an organization are, first, that it shall possess sufficient dignity and authority to make its direction respected, and second, that it shall have at its disposal sufficient funds to pay for the expenses of the work. Several possible ways may be suggested for developing such a mechanism for cooperative research. In the first place it is quite possible that any body of scientific men may cooperate by a series of conferences and some sort of a voluntary compact. A notable example of an important effort of this kind is found in the International Union for Cooperation in Solar Research, already referred to. Quite

recently also the National Academy of Sciences, through a special committee, has taken steps to organize a definite plan for cooperative research upon the equilibrium conditions of chemical reactions. A similar plan might be followed with advantage by any or all of our special societies. Speaking for my own subject, there is no reason why the American Physiological Society should not, through its council or by means of special committees, plan out work of a general character and enlist the cooperation of selected investigators. There are a number of questions in physiology which bear upon public health or social conditions which might be studied systematically in this way. There is an important field also in the determination of physiological constants and the standardization of methods and apparatus which might be worked better by this method than by the accidental cooperation of individual investigators. There can be no doubt that such an effort would be well worth making even if it fell short of the full measure of success hoped for. Some data of fundamental importance would be obtained with a degree of completeness and certainty which could hardly be reached by any other method. There is another consideration of subsidiary importance which is worthy of passing notice in this connection. It is, I believe, a matter of common knowledge that in every department of science there are many able workers who remain unproductive because of a certain lack of initiative, or because they waste their time and opportunities in ill-directed efforts. Quite often these workers are the very ones who have had the most careful training in technique and are the best qualified to accomplish difficult research work. If under the influence of some central organizing force they could be enlisted in a systematic campaign of work, their training would be utilized for the benefit of science and to their own best interests. There is another class of workers, to be sure, who are so constructed temperamentally that they never accomplish their best work except as free lances—for them cooperation would be irksome and deadening. In any such plan of work as that contemplated some discretion in the selection of workers would have to be exercised by those charged with the general direction. I am convinced, however, that an earnest persistent effort to organize cooperative work is well worth making on the part of all of our scientific societies. It goes without saying that a voluntary cooperation of this character would meet with many partial failures; much that was initiated might fail to run a completed course, owing mainly to the lack of a compelling sense of obligation on the part of those entrusted with the details of the work, but on the principle that half a loaf is better than none I believe that we should all do well to follow the example set us by the astronomers. Another source to which we might look for aid in developing and testing the cooperative method is found in those large scientific bodies which have a certain amount of money at their disposal for the encouragement of research. In some cases the money controlled by these societies has been given for specific purposes and would, therefore, be difficult to administer in the way here suggested. More frequently, however, the funds are available for the promotion of scientific knowledge in general by means of investigations. As a rule such funds are disbursed on the principle of competition rather than of cooperation. They are used to subsidize individual researches, and the work accomplished, however good it may be in the single piece, is scattered over a wide field and lacks the effectiveness which might be obtained by intelligent super-

vision. The method of subsidizing is a method fitted to encourage or perhaps to discover the individual worker of talent, rather than to promote an increase in knowledge. While it has much more to commend it than the wasteful and almost useless system of granting prizes, we must admit that in its actual working it is haphazard; a blind sowing of seed, the harvest from which depends largely upon chance and circumstances. One may be allowed to question, therefore, whether it might not be more productive of good, if societies with funds entrusted to their keeping, such as the National Academy of Sciences, would make an effort to dispose of their funds in the systematic investigation of fundamental problems. The society mentioned has at hand, in its own membership, men who are abundantly qualified to select the right problems and to direct and coordinate the work of those entrusted with the several investigations. Whether such a use of its funds is permissible I can not say, but if such is the case one can scarcely doubt that by organizing systematic research of a cooperative character the National Academy could make itself a living and stimulating force in the scientific activity of this country. But among the agencies to which we may look for help in the matter of cooperative work, the two which seem best adapted for this variety of research are the laboratories supported by the government and the specially endowed institutions of the type of the Carnegie, Rockefeller, Wistar, etc. In regard to the governmental laboratories it is natural to suppose that the problems to which their resources might be applied most appropriately are those possessing an immediate economic importance. Individual scientists in the service of the government have without doubt contributed many investigations of the first importance, as they would have done under any circumstances which offered them equal facilities for work. But the specific function to which these departments are best adapted would seem to be the prosecution of investigations bearing more or less directly upon the health and wealth of the citizens of the country. I do not mean to say that it is inappropriate for the government to give its support to investigations of the more fundamental and theoretical problems of science, but at present, at least, funds from this source can probably be obtained with least opposition when the work undertaken gives promise of a more or less immediate application to the needs of life. In following out such investigations the laboratories of the government are peculiarly fitted by their organization to effect a coordination of the labors of their individual workers. On the contrary, the specially endowed institutions have a freer hand in the disposition of their resources and are less hampered by the necessity of adopting a utilitarian policy. With large means at their command and with a centralized authority, fitted to direct and control the investigations made by their scientific staffs, these institutions constitute ideal mechanisms for testing the effectiveness of cooperative research—it would seem, indeed, that in this field there lies for them an especial opportunity. The laboratories of our universities form training schools wherein young men and women must be taught to use the appliances of research, and it is almost a necessity of the case that the work shall be large and varied. The whole range of a given science should be presented and exemplified as far as possible. In these laboratories also the opportunities for individual research should be made as wide as possible—therein lies their special mission, and as a matter of fact this condition prevails at present, and has prevailed from the beginning of scientific

laboratories connected with academic institutions. If our specially endowed institutions simply follow the same general plan they will add nothing distinctive to the character of the scientific activity of the country. It will be as though one or more new universities had been organized, and the present opportunities and methods had been somewhat extended—a chance for a few more investigators to try their powers under conditions not materially different from those already existing in many laboratories. If, on the contrary, the energies and appliances of these institutions were directed toward a cooperative concentration of effort, then indeed, they would fill a need not now efficiently met by any of our existing scientific foundations. There seems to be no reason why the directors in such institutions should not exercise the power of planning a campaign of work in which all the talent and training of the workers under their control should be brought to bear upon a systematic continuous investigation from several sides of problems of importance. The policy that seems to have been adopted by the Carnegie Institution, of applying its funds to the creation and maintenance of special laboratories, such as the laboratory of nutrition and the Desert Botanical Laboratory is a welcome step in this direction. Well equipped and well directed, they will accumulate data of the greatest importance and will fulfill a function which our teaching laboratories, by their organization, are unfitted to exercise. Laboratories of this character so organized that their forces can be coordinated now upon one problem and again upon another constitute a kind of machinery which is at present lacking in our scientific workshop and from which results of the greatest value may reasonably be expected.

WILLIAM H. HOWELL
THE JOHNS HOPKINS UNIVERSITY

THE topic before us for discussion permits of a great variety of interpretation. It would be feasible and interesting to discuss the possibilities of cooperative endeavor on the part of university departments of psychology on the one hand, and the medical departments of mental and nervous pathology on the other. Such cooperative enterprises have already been given a trial in one form or another both in this country and abroad, and so far as I am aware, with general satisfaction to all concerned. A much further development, however, is practicable, and one of the first steps in this direction, already taken by the more progressive medical schools, consists in the requirement that medical students should familiarize themselves with the rudiments at least of modern psychology. In my judgment this movement is but an expression of the most obvious common sense and I welcome it as such; but I am sure that much remains for the psychologist to do even on this level of cooperation in supplying the medical student with a selected material peculiarly appropriate to his needs.

We might also discuss the possibilities of cooperation between the departments of neurology and psychology. This is a hobby which I am glad to ride at any time. Again we might with advantage consider the possibilities of cooperative division of the field of study dealing with animal behavior as between the zoologists and the animal psychologists. But it is clear that within the limits of time at my disposal any such discussions would monopolize my part of this program and at the end find us with only one of many equally important groups of relations examined. I have therefore chosen to devote myself to a consideration of the special demands which psychology has to make upon several of the biological sciences and to a brief statement

of some of the contributions which it is in position to offer in return for favors received. I choose this course in the hope of furthering among the distinguished naturalists here present a fuller understanding and appreciation of the ideals, aims, and necessities of modern psychology.

In emphasizing the *demands* psychology makes I am by no means oblivious to the inestimable services already rendered her by the natural sciences, but I may safely assume that the more important of these services are familiar to you and I prefer to attempt to make vivid the future favors for which we look.

It should be remembered at the outset that historically psychology arose out of philosophy, and it still retains a large measure of intimate filial relations with its first parent. In recent years, however, it has been increasingly adopting the manners and point of view of its brothers and sisters, the natural sciences. This has been particularly true since the introduction into psychology of experimental methods. Although there are not at present any sharp lines of division between what may be called philosophical psychology and psychology as a natural science, the distinction in emphasis is none the less real. I shall, for the present purpose, disregard the more philosophical branches of psychology and confine myself to those of a more scientific character whose relations to the biological sciences are necessarily most intimate.

It should perhaps be added that the psychology of this stripe conceives its business as the study of the organization and operations of mind; broadly and metaphorically, this field may be called the anatomy and physiology of mind. Moreover, it regards the mind not as a remote, abstract metaphysical entity, but as a concrete vital function bound up in the most intimate connection with physiological processes which must be taken into account before it can be properly and fully understood.

Perhaps the most persistent and important levies which psychology undertakes to make are at the expense of neurology, using this term to designate the scientific study of both the anatomy and physiology of the nerves, it being understood that this includes certain phases of physiological physics and chemistry. The situation here is so obvious and so familiar as to require little elaboration. For modern psychology, the hypothesis that the mind is functionally dependent upon the nervous system has become substantially a postulate. Whatever we can learn, therefore, about the nervous system, is clear gain to us in our efforts to disentangle the complexity of mental life and to apprehend its principles of organization. We wish to know everything which the neurologists can tell us regarding the ways in which nervous currents act, how they reinforce one another, how they are inhibited, and what are the conditions of their original arousal. We wish to know also, more completely than we now do, what the great typical pathways are through the nervous system and what junction arrangements these pathways have with one another. These facts we wish for the light which they may throw upon certain recognized peculiarities of mental process. What explanation, for example, can the neurologist offer us for such peculiar facts as are disclosed by the synæsthesias, in which the stimulation of one sense organ like the ear is immediately followed by sensations not only of sound but of color or taste? What explanation can he offer for the fact that the color threshold is lowered by the simultaneous stimulation of the ear?

We should like to know what occurs as

regards neurone transmission when an act previously carried on with painstaking, conscious effort, becomes so habitual and automatic that conscious control can be, and often is practically, done away with. Does this involve the use of pathways below the level of the cerebral cortex, or does it simply mean decreased resistance in cortical neurone systems, consciousness being the expression of such resistance? Our basal conception of the relation of consciousness to the nervous system will be determined in no small measure by a conclusive answer to this question. Despite much dogmatic assertion and certain very interesting recent experimental investigations, the problem is still unsolved.

We wish also and particularly to know what portions of the nervous system, or what modes of nervous action are primarily responsible for the great subdivisions of our mental life. In a rough way we have already learned something in reply to such questions regarding the portions of our brain responsible for certain of our sensations and movements and even for certain of our ideational activities. But the details are still very hazy. We should be glad for a more definite knowledge of the differences in cortical action which distinguish sensations and perceptions from ideas and images. Here again dogmatism runs far in advance of well-organized and demonstrable fact. We should also welcome most enthusiastically any fundamental illumination as to the physiological basis of memory.

We want to know much more than the specialist can now tell us about the structure and function of sense organs. Behavior is simply a generic term for muscular movements made originally in response to sensory stimulations of one kind or another. It is obvious that we must know the characteristics of these sensory excitations before we can adequately understand the reactions which are made to them. This is peculiarly true of comparative psychology, with its interest in the mental processes of animals, but it is equally true at bottom in the case of human psychology. We want, for example, to know the structure of the retina in the lower animals and especially those whose vision is not binocularly unitary, and we also want to know the facts about the visual conduction pathways in the central nervous system in the case of such animals. This knowledge we desire, not only to enable us more exactly to interpret the behavior of such animals but also, and particularly, for the light which it may throw upon human visual processes. That animals have eyes might seem to imply that they see colors and yet, as is well known to you, evidence is rapidly accumulating to render it fairly certain that many animals supposedly possessed of color vision, in reality are sensitive only to differences in brightness or luminosity. A careful examination of the retinæ of such animals may well give us our long needed clue wherewith to untangle the puzzles of the human color sense. In the case of man it seems not unlikely that a completely adequate color theory must await researches in physiological chemistry as yet unmade. Nevertheless, we may get our start from such investigations as these just suggested upon the animal retina.

I need hardly add that a correct interpretation of animal behavior depends upon the solution of problems such as these: for example, much evidence is now at hand indicating that animals may possess developed sense organs of which they make little or no actual use under normal conditions. It is wholly problematic whether certain birds make any actual use of smell as a sense process, and yet the evidence suggests that anatomically they are equipped to respond to odors. Similarly,

certain varieties of rats make under many conditions little or no use of their eyes. For such animals these senses are almost as much of a luxury as the vermiform appendix is for man. The naïve observer of these animals, unfamiliar with these peculiar facts about the non-use of these sense-organs, must inevitably go astray in interpreting their reactions.

We can not hope for a thoroughly satisfactory theory of auditory consciousness until we learn more adequately the anatomical and physiological facts about the cochlea. Of late the widely accepted Helmholtzian theory of sympathetic resonance by the basilar membrane fibers, has received some almost fatal wounds and none of the substitutes as yet proposed is wholly convincing. I might unfold a similar tale of defective present-day knowledge in the case of each of the senses, and the psychologist stands ready and eager to appropriate with gratitude whatever can be given him here. Are there without doubt special end-organs for the temperature sensations, and for the several forms of contact? What is the implication as to our bodily sensations of Head's recent experiments on sensory nerve regeneration and the return of sensitivity after nerve section? Are there specialized end-organs for the four elementary tastes whose psychological and physiological distinctness seem so certain? Is there no differentiation in the olfactory end-organ comparable with the bewildering profusion of olfactory sense qualities? The answer to these and to dozens of other similar questions must be obtained before psychology can be satisfied with the finality of its analyses and explanations of sensory consciousness.

From comparative anatomy and physiology, as well as from embryology, we look for much helpful light on the circumstances surrounding the appearance and growth of intelligence. Time fails me, however, to attempt to specify details.

Modern psychology gladly acknowledges a great debt of gratitude to the alienist and the pathologist. The study of insanity and nervous diseases, chaotic as are the present conditions in those branches of medicine, has been of indisputable moment to psychologists. Moreover, we recognize that the studies of the neurologists and the pathologists are mutually indispensable to one another and that answers to many of our questions already formulated must come from both these scientists in order to be complete. The study of alternating personalities, of hypnotic phenomena, of somnambulism, and the positive insanities, to mention only these, has let in a flood of light upon the complexities of organization in the mental machinery which could not otherwise have been attained. The questions which the psychologist still has to put to these colleagues of his are so numerous as wholly to baffle summary. They can only be illustrated.

Is the disintegration of the self found in the so-called alternating personalities simply an exaggeration of normal conditions, or is it wholly pathological in the sense in which scarlet fever is? Already intimations of the final answer to this question are looming large. What is the origin and inner character of the so-called "phobias," so characteristic of our day? This man is a neurotic hypochondriac; that man a neurotic recluse. Have these diseases their foundation in specific lesions of one or another kind; are they expressions of hypertrophy of normal physiological functions, or are they purely psychic? What is the physiological basis of suggestion often employed in treating such conditions? Perhaps, if a satisfactory reply could be obtained to this last question, our medical friends would be less generally willing to hand over to Christian

Science and Faith Cure healers the undeniable therapeutic values of this process. Medicine, as well as psychology, could therefore profit by the answer. What is the physiological foundation of hypnosis? Answers to questions such as these would set us far on our way to a better understanding of the mind and its connection with the body. With due modesty I may as a psychologist say that the issues raised here concern matters about which our present knowledge is almost exclusively psychological.

I trust that in this brief sketch I have made it clear that psychologists are watching with utmost eagerness a wide range of neighboring scientific territory from which they will purloin anything of value to them if not prevented, and I hope I have also shown that their needs are many and genuine and definite. But what has the psychologist to offer in return for the blessings of natural science past and future?

I shall make my reply very brief and confine it to a few words dealing first with the general advantages which psychology offers and second, to the specification of a few more concrete details of service.

One very obvious and simple service which the psychologist would be glad to render his scientific colleagues is the tender of a knowledge of a few simple psychological distinctions and a reasonably satisfactory terminology in which to clothe them. It is depressing to the psychologist to find his brethren still using ideas and terms which were becoming obsolete in psychology at the beginning of the nineteenth century. It may readily be granted that the terminology created for strictly psychological purposes may be found unsatisfactory in some particulars when employed in psychiatry or neurology. But the correct alternative to choose in the face of this difficulty would not seem to be the naïve creation of a new terminology, nor the utilizing of one already outworn, but rather the modification of the best one in vogue. I would not seem to imply that psychological terminology is a finished and satisfactory product. Quite the reverse is true, but it has some relatively stable features to offer and some good reasons for offering them. Moreover, there are certain elementary psychological ideas and principles which are quite firmly established, and should be familiar to every scientist whose work requires him now and again to indulge in psychological statements. Such terms as sensation, perception, imagination, memory, attention, association, conception, reasoning, emotion, and volition have sufficiently fixed and definite meanings attached to them to render their use perfectly practicable. Without such knowledge it is hardly possible to make any extended statement about mental facts without becoming involved in needless terminological difficulties.

I trust my attitude will not be misunderstood. I speak in sadness, not in irritation; in sorrow, not in anger. Open the standard anatomies of the nervous system and you will not infrequently find diagrams of the cerebral cortex with one set of areas marked "sensory" and another set marked "psychic," as though sensations were not psychic and as though psychic meant anything in particular anyhow. What kind of psychic? Emotional psychic; ideational psychic; volitional psychic? To classify the functions of a region as psychic is much like classifying the people of the United States as human. It may be true but it is not illuminating; and if the term psychic is employed as significant simply of something not immediately sensory in character it is an unpardonably vague term for which good substitutes are easily available in psychology. If it is used as a cloak for ignor-

ance, then the term "*unknown*" should be substituted for it in the regions concerned.

There are many general advantages of a similar character to be gained by the scientist from a slight acquaintance with psychology, and not the least of these is perhaps the more vivid appreciation on his part of the elaborate technique which modern psychology has worked out to meet her needs and the substantial foundation which now underlies modern psychological doctrines. In so enlightened a body of scientists as this which I now have the honor to address, there is undoubtedly no such shallow misconception of the attainments of modern psychology, but there are many who still dwell in the darkness of intellectual night so far as concerns this matter.

I shall select simply a point or two to illustrate the more specific and particular ways in which psychology may contribute to the natural sciences. The contemporary naturalist often has occasion to make use of the psychological principle of association and I would urge on his thoughtful consideration the psychological analyses of this feature of mental life. The bland naiveté with which he often uses this principle makes one gasp who has ever faced its multitudinous complexities. It is a safe surmise that Aristotle had forgotten more about the principle of association than certain modern naturalists have ever known. It is respectfully submitted that it is not good common sense in the use of a principle like this wholly to disregard the elaborate analyses of generations of previous workers. Again, it is out of the question for the neurologist, for instance, studying the function of the auditory end-organ apparatus to go far or safely without a knowledge of such generally unfamiliar phenomena as those of combination tones with their many varieties. Similarly the physiology of the visual processes must remain lamentably incomplete in the hands of an investigator unfamiliar with the important facts of color vision: for example, the peculiarities of such vision under dark and light adaptations respectively, the phenomena of contrast, peripheral retinal color deficiency, the peculiarities of peripheral and foveal space impressions, and so on. In other words, psychology is in a position to furnish a systematized statement of vast ranges of mental phenomena which not only *may* be taken into account by the neurologist, but which *must* be taken into account before his science can approach completion, because these phenomena constitute many of the concrete facts which it is his business to explain. In other words, psychology—or some other science doing her work—sets many of the most important problems for the other biological sciences. Facts which she finds, they must take account of and, if possible, explain.

James Rowland Angell
University of Chicago

SCIENTIFIC BOOKS

The Value of Science. By H. Poincaré, Member of the Institute of France. Authorized Translation with an Introduction, by George Bruce Halsted, Ph.D., F.R.A.S. With a Special Prefatory Essay. Pp. iv + 147. New York, The Science Press. 1907.

In calling attention to M. Poincaré's masterly little book, I propose—these columns being what they are—to consider rather its general significance than to traverse the technical problems of logic and epistemology which it raises. For scientific workers at large, the *tendency* of the monograph happens to be the most important thing about it. It adds another to the numerous contemporary evidences that scientific investigation, when subjected to reflection, and viewed with regard to its methodology and intellectual presuppositions, leads unavoidably to difficulties that belong in the field of philosophy. No doubt, I may incline to exaggerate this view, but, as

I have preached it for years, when another tide was running, I am accumulating no fresh sins on my head, now hoary with them!

Since the days when Lavoisier dethroned phlogiston, Black founded his kitchen-laboratory, Schleiden and Schwann enunciated the cell-theory, Helmholtz published his epoch-making paper on energy, and Pasteur discovered chiralitry, we have been so occupied in the detail of scientific acquisition that little time and, perhaps, less appetite remained for inquiry concerning the fundamental principles in human consciousness whereon all discovery is based ultimately. Doubtless some did philosophize, like Lotze and Mill, and even E. du Bois-Reymond; but in the rush of new and ever new knowledge, they fell upon neglect, or their speculations consorted, in most minds, with other curious diversions. So, fate working with irony as always, many were content to wallow mid most serbonian bogs, hidden away in the recesses of mental construction and, in the eyes of the too few elect, contrived to cut sad antics. The fine futilities of agnosticism, the unashamed, because unconscious, contradictions of materialism, and the mystic improprieties of hylozoism thus came to do duty as presentable accounts of first principles. Anything "went." Every student of the history of culture knows perfectly well that this sort of thing can not go on indefinitely. A day of reckoning has arrived invariably, later if not sooner; and there is no reason to surmise that our own case will furnish any exception to a constant rule. If we would tarry only long enough to ask, for example, the simple question, To what does hypothesis amount? we would, beyond peradventure, rub our eyes when confronted with the unexpected result. It is well, therefore, that a past master in one realm of science should have taken heart of grace to call a halt for the purpose of reviewing some of the fundamental presuppositions incident to all phenomenal research. M. Poincaré's troubles may, indeed, seem far removed from the daily storm and stress of our laboratories. Nevertheless, they can not be dodged if one would know what reliance can be placed upon that elusive thing we label confidently "scientific certainty."

Now, obviously, when normative, and therefore very general, problems come in question, the thinkers who attack them will be influenced, severally, by previous interest, familiarities, and consequent constructive intention. They are helpless to rid themselves of distinctive standpoint. Here, if anywhere, we perceive that the "human" is himself the most important piece of apparatus in the laboratory. We must not expect Poincaré and Ostwald, for instance, to stress identical differences, or even to approach the same issues with similar intent. Nay, even naturalists, like Brooks and Arthur Thomson, diverge widely both in method and outlook, when they record their conclusions on first principles. Naturally, then, M. Poincaré proves this rule —he is always the mathematician, and the mathematician of most "modern port." For this very reason his work proves enthralling, even if some young lions of philosophy could pierce his speculative guard here and there.

Nor is this all. The mathematical spirit comes permeated by gallic genius. Where we, and our kith in the British isles, achieve results by vast compilation of examples, where we are valiantly empirical, the French proceed by way of abstraction and quick appeal to rational principles. Knowledge must fetch and carry for us; for them she is a mistress to be worshipped with a kind of holy joy. Ideas render us restive or impatient, the French would die for a "cause." Hence, as Glazebrook records, in his monograph on Maxwell (pp. 216 f.), Poincaré experiences "a feeling of uneasiness, often even of distrust," in approaching Maxwell's investigations, because "Maxwell does not give a mechanical explanation of electricity and magnetism, he is only concerned to show that such an explanation is possible." The canny Scot was not there to buy out the entire store; he would go in and ask change for sixpence! And he would get it, moreover. But, what is the basis for his procedure? Ignorant of this, his action can not be grasped. The philosophical instinct, with its ineradicable suspicion so intolerable to the eager researcher, speaks

here, and the vital question of legitimacy of method looms up.

M. Poincaré's tendencies in this connection are familiar already to readers of, say, his "Electricité et Optique," or "La Théorie de Lorenz et le Principe de Réaction" (*Arch. néerland.*, ser. 2, v. 5), where his criticism of mechanical constructs ends in the affirmation that, not these, but *unity*, do men really seek. In a word, the empirical references of mechanics must be expressed in mental terms if we would estimate their value for a human experient. The way in which men regard things, the way of thought, calls for consideration just as close as the grasp they may have obtained upon particular objects. In fact, thus the value of alleged grasp must needs be estimated. In a more concrete sense than Grassmann's, and with no necessary reference to prudence, "a doctrine of Forms should precede a doctrine of Magnitude," as H. Hankel pointed out forty years ago. In short, two questions, long subordinated, thanks to preoccupation in special discovery, thrust themselves forward. What basis does scientific thought possess in the sphere of logical reasoning? What value can be assigned to scientific thought in the complexus of human experience? That is to say, M. Poincaré confronts the Sphinx, who asks, What validity, if any, does the scientific view of the universe hold in its own right. And, naturally, his interest being what it is, his achievements being what they are, he presupposes the work of such earlier masters as Riemann and Weierstrass, of such recent scholars as Kronecker, Paul du Bois-Reymond and F. Klein, to say nothing of the remarkable group of his own fellow countrymen. I mean his approach is from this side, not from that of the philosopher *pur sang*. But this matters little, for he has been gifted with a double portion of that Gallic wit which, in our time, stands for Attic salt—the wit to refine ideas of all dross, and to present them crisp from the crucible of thought.

The contrasted, yet complementary, nature of the labors of Riemann and Weierstrass, as noted by M. Poincaré himself (*Acta Math.*, xxii.), serves to hint the general scope of the problem with which he wrestles in this book. "By the instrument of Riemann we see at a glance the general aspect of things—like a traveler who is examining from the peak of a mountain the topography of the plain which he is going to visit, and is finding his bearings. By the instruments of Weierstrass analysis will, in due course, throw light into every corner, and make absolute clearness shine forth." What is this but the age-old puzzle of the universal and the particular? What kind of author have we but one who, being a marvelous analyst, is also an ornament of the school of synthetic mathematics? And we must be prepared to learn, accordingly, that rule of thumb may turn out no rule. A theory may never render a more valuable service to science than when it breaks down, as M. Poincaré has himself said ("La Science et l'Hypothèse," p. 170).

M. Poincaré's conclusions are dominated by considerations like the following: "A reality completely independent of the mind which conceives it, sees or feels it, is an impossibility" (p. 14). "We have not a direct intuition of simultaneity, nor of the equality of two durations. If we think we have this intuition, this is an illusion. We replace it by the aid of certain rules which we apply almost always without taking count of them" (pp. 35–6).

"Space is a mathematical continuum, it is infinite, and we can represent it to ourselves only by physical continua and finite objects. . . . Absolute space is nonsense" (p. 56). "Experience does not prove to us that space has three dimensions; it only proves to us that it is convenient to attribute three to it, because thus the number of fillips is reduced to a minimum" (p. 69). "I believe, therefore, that if by space is understood a mathematical continuum of three dimensions, were it otherwise amorphous, it is the mind which constructs it, but does not construct it out of nothing; it needs materials and models" (p. 72). "The invariant laws are the relations between the crude facts, while the relations between the 'scientific facts' remain always dependent on certain conventions" (p. 128).

"A philosopher really anti-intellectualistic

is impossible" (p. 114). *"All the scientist creates in a fact is the language in which he enunciates it"* (p. 121). "Since the enunciation of our laws may vary with the conventions we adopt, since these conventions may modify even the natural relations of these laws, is there in the manifold of these laws something independent of these conventions and which may, so to speak, play the rôle of the *universal invariant*? . . . In any case a minimum of humanity is necessary" (pp. 127–8). "All classification supposes the active intervention of the classifier" (p. 135). "Sensations are therefore intransmissible, or rather all that is pure quality in them is intransmissible and forever impenetrable. But it is not the same with relations between these sensations" (p. 136). "Nothing is objective except what is identical for all; now we can only speak of such an identity if comparison is possible, and can be translated into a 'money of exchange' capable of transmission from one mind to another. Nothing, therefore, will have objective value except what is transmissible by 'discourse,' that is, intelligible" (p. 137). "All that is not thought is pure nothingness; since we can think only thought and all the words we use to speak of things can express only thoughts, to say there is something other than thought is, therefore, an affirmation which can have no meaning" (p. 142).

It is plain enough, from these representative and characteristic selections, that M. Poincaré has not acquired familiarity with psychological investigation; that, as yet, he has not compelled himself to think through to a definite, coordinated, basis in epistemology; that his logical methods tend to gloss the secondary character of symbolism; and, above all, that he has not clarified the ultimate metaphysical problem immanent in his acute dialectics. But of these limitations, as the professional philosopher will at once see them, I incline to make light. For it is an immense gain that M. Poincaré should have insisted, not merely upon the existence of such riddles, but upon their fundamental import for an evaluation of scientific modes of presentation.

The book ought to be in the hands of all who desire to "mix their colors with brains."

R. M. WENLEY
UNIVERSITY OF MICHIGAN

Atlas of Absorption Spectra. By H. S. UHLER and R. W. WOOD. Carnegie Institution of Washington, Washington, D. C., 1907.

"To furnish graphical representations, on a normal scale of wave-lengths, of the absorption spectra, both in the visible and ultraviolet regions, of a reasonably large number of compounds," is stated by the authors as their chief object in producing this book, and with the exception of the fact that their spectrograms do not extend into the red, their object has been very well attained.

The book opens with a two-page introduction by Professor Wood, which is followed by eighteen pages including descriptions of the apparatus used, spectrograph, sources of light, photographic materials, explanation of the tables, etc. The tables occupy about forty pages, and give, in systematic form, the results obtained for 147 aniline dyes and some of their related organic compounds, and 36 miscellaneous absorbing media, chiefly inorganic salts. Twenty-six plates, 102 figures, positives of the spectra observed, complete the book.

The dispersing apparatus used was a concave grating of 98.3 cm. radius, the ruled surface of which was 1.96 cm. by 5.36 cm. Most of the photographs were taken on celluloid films, sensitized with Seed's "L-ortho" emulsion. A few photographs were taken on Cramer's Trichromatic plates, for the orange and red regions up to about .63 μ. Most of the plates extend from about .20 μ or .22 μ to about .59 μ or .60 μ, where the Seed plates cease to be sensitive for normal exposures.

A Nernst glower carrying .8 ampere on a 104 volt 133 cycle circuit, furnished a continuous spectrum down to about .32 μ or .34 μ. A spark between electrodes, one of sheet brass and the other of equal parts of zinc and cadmium, furnished a bright line spectrum from about .2 μ up. The spark spectrum was cut off from the plate by a movable screen

for all wave-lengths greater than .4 μ so that the second order ultra-violet might not overlap the continuous spectrum. The spectrum of the Nernst glower was too weak to affect the plates at .325 μ in the first order, and could not therefore have produced any effect below .650 μ by the overlapping of the second order. A 75-second exposure was usually given to the spark, then the screen was removed and a one-minute exposure given to the spectrum of the Nernst glower. The spark terminals were made very broad and chisel-shaped and their edges were placed parallel to the slit, a considerable length (about a centimeter), of which was thus illuminated. A large Leyden jar was connected across the spark gap.

An ingenious form of cell for holding the absorbing liquids is described in full. By means of it a wedge-shaped film of liquid was confined between quartz plates which could be set at any desired angle to each other and at any distance apart up to 6 mm. When properly placed before the slit the light coming to each successive point of the slit came through successively increasing thicknesses of the dye. Three exposures were usually made on each plate, their edges nearly in contact and the angle of the wedge and its position at each exposure was such that the three photographic strips showed, from the top of the first to the bottom of the last, the effect of a continuously increasing thickness of the absorbing film. The thickness at one edge was zero and at the other usually about .25 mm., although the thickness at the thicker edge varied over a considerable range.

The authors have anticipated the chief criticisms which might have been advanced against the book. In stating their "chief object" they disclaim any intention to attempt quantitative measurements or to make an exhaustive study of all known dyes. They have certainly investigated "a reasonably large number of compounds." Their statement that "only aqueous solutions of the aniline dyes have been investigated up to the present time" leads us to hope that the investigation will be continued both for other solvents and for other dyes, including some of the very important new photographic dyes which are not in their present list.

No attempt has been made to give to a high degree of exactness the positions of the absorption bands, and it is doubtful if either this or the relative strengths of the absorption bands could have been found satisfactorily with the commercial plates used. And it is perhaps because no attempt in this direction was intended that no data are given as to time of development, temperature of developing bath, etc.—nor are we told whether or not any attempt was made to secure uniformity in these respects.

The authors give ample warning that the photographic minimum in the Seed plates used may produce apparent absorption in the green. Without doubt there is much of interest in the red of many of these dyes, and it is to be regretted that the authors did not make use of some of the modern methods of plate bathing, or even of some of the later plates now obtainable commercially, both to secure a more uniform photographic sensitiveness throughout the spectrum, and to extend the observations into the red. As they point out, however, the slope of the limiting line at the red end of their plates indicates whether or not an absorption band is present in that region.

The book is very well printed and the plates seem to be excellent, although they do not seem to show all that may be seen on the original negatives, as is evidenced by a comparison of the tabulated data of some of the figures with the figures themselves, *e. g.*, in the case of potassium permanganate, Fig. 75, we are told that the negative shows seven absorption bands in and near the green. Only five of the seven can be detected in Fig. 74. The absence of typographical errors is noticeable. Under Fig. 99, however, we are referred to page 169, instead of, evidently, page 59.

Henry G. Gale
Ryerson Physical Laboratory

Societies and Academies

The Oregon State Academy of Sciences

The third annual meeting of the Oregon State Academy of Sciences was held at the

Oregon Agricultural College on January 17 and 18. Four sessions were occupied in the presentations of papers.

A reception was given to the academy members by the faculty of the Oregon Agricultural College on the evening of the seventeenth.

Papers were presented as follows:

"South African Flora," by Mary F. Farnham.

"Oysters in Oregon," by A. R. Sweetser.

"Future of Mining," by A. C. Terrill.

"Space and Number Systems," by H. B. Leonard.

"Theory of Electrons," by L. A. Robinson.

"Surface Tension applied to Ore Dressing," by H. M. Parks.

"The Birds of the Three Arch Rock Regions," by Wm. L. Finley.

"Apple Tree Anthracnose," by C. C. Cate.

"Notes on Trichoptera," by Annie Laura Hill.

"The Tides," by J. D. Lee.

Officers for the ensuing year were elected as follows:

President—A. R. Sweetser.
First Vice-president—A. T. Bohman.
Second Vice-president—A. B. Cordley.
Recording Secretary—C. E. Bradley.
Treasurer—C. O. Chambers.
Trustees—Dr. J. Withycombe, T. C. Bridwell and P. L. Campbell.
Librarian—A. W. Miller.

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

THE paper of the 412th meeting was by Dr. George Byron Gordon, Free Museum of Science and Art, Philadelphia, on "An Ethnological Survey of the Kuskokwim River, Alaska." Dr. Gordon said that in continuation of his Alaskan explorations begun in 1905 he visited during the summer of 1907 the region of the upper Kuskokwim River, and embarking on that stream, descended its whole length to the mouth in Bering Sea. The upper river for 200 miles he found to be untenanted by man, and it appears that there is a corresponding scarcity of animal life. The first habitations reached were abandoned, and in one house five dead bodies were lying as though overtaken with a sudden pestilence, and later it was ascertained that virulent pneumonia had swept the valley, almost exterminating the natives. Lower down the river an uninhabited village was reached, and Dr. Gordon observed that the people were of Eskimo type, but spoke a Tinne Indian dialect. Their houses are of logs, stood up in arch-shape and covered with earth. Assembly or club-houses of large size exist here, and there are numerous caches, graves and salmon-drying racks. The caribou, on which the natives depended, have left their former range and do not now visit the Kuskokwim. Dr. Gordon visited the Eskimo village at the mouth of the river and secured photographs, measurements and other data concerning the people. The inroads of disease among the natives, says Dr. Gordon, are frightful, and in a few years it is possible that the inhabitants of this region will be exterminated by maladies introduced by whites. Dr. Gordon said, in answer to a question by Mr. Robinson, that the timbers of the old houses on the upper Kuskokwim had been cut with ivory and stone tools. The discussion of Dr. Gordon's highly interesting paper was participated in by Messrs. Heye, Robinson, Hrdlicka and others.

WALTER HOUGH,
General Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 642d meeting of the society was held on January 4, 1908, President Bauer in the chair.

By invitation, Mr. Percival Lowell read a very interesting paper on the subject of "The Recent Discoveries at Flagstaff in Saturn's Rings." In June, 1907, a new phenomenon was observed in Saturn's rings, at Flagstaff. At this time the plane of the planet's rings was directed towards the earth and the interesting thing was the band. The dark chord or core seen in the band at this time was the phenomenon that had not been previously known or observed. The same phenomenon was again seen in October last by all of the observers at Flagstaff, and careful measurements were made of the band, the dark medial chord and of the positions (in terms of the planet's radius) of the several luminous appendages.

It was shown by the speaker that the observed width of the band was too great to be explained by the rings' shadow or by their

penumbra and it was stated that its width could be accounted for by supposing the existence of matter outside of the plane of the rings, or in effect by tores or thickenings of the rings; the dark medial core of the band being the projection of the ring proper. It was explained in what way the tores could have been produced and in what part of the rings they should occur. In this connection it was shown how the separations of the rings were produced by the planet's satellites; and it was stated that one should expect that the tores should occur just inside of the paths of the satellites. By observation the tores were found to be just where celestial mechanics would put them under the disturbing influence of the satellites.

In reviewing the conclusions as to the constitution of the rings, and the disintegration that must be taking place as evidenced by the positions of the tores, the ultimate disappearance of the rings was predicted.

R. L. Faris,
Secretary

DISCUSSION AND CORRESPONDENCE

AGE OF A COOLING GLOBE

To the Editor of Science: In my paper on a cooling globe in Science for February 7, pages 231 and 232, the depth of the level of isostatic compensation is stated as 71 miles or 140 kilometers. This last should be 114 kilometers. The blunder arose in copying, and the correct value was used in the computations.

George F. Becker

THE EARTH AS A HEAT-RADIATING PLANET

To the Editor of Science: Of the many far-reaching consequences resulting from the discovery announced in Science for November 22 and December 20, 1907, perhaps no one fact stands out more clearly and strongly than this—*The inherent heat of the earth still plays an important if not controlling part in all terrestrial phenomena* (as, for instance, in the formation of ocean and atmospheric currents, in cloud formation, and the increase in temperature with increase of cloudiness, etc.), *for it now seems certain that without this inherent heat radiation the terrestrial atmosphere could not exist.*

With a surface which, even at its lowest known temperature, is still more than 200° C. above the temperature of surrounding space (ocean temperatures at great depths being about 270° above) and with the temperature increasing with the depth below the surface, there can no longer be much reasonable doubt as to the facts concerning the past or future history of the earth, so far as effects due to temperature changes are concerned.

The earth is now, and has been for ages, radiating heat into space, shrinking in size, and, with a constantly decreasing surface temperature, growing colder.

The mean absolute surface temperature of the earth is, let us say, 300° C. If we regard this as made up of an inherent surface-temperature of 200° C. and a stored or trapped heat equivalent to a temperature of 100° C., the radiation into space is such that the moon, for example, receives about one twenty-seventh as much heat from the earth as it does from the sun.

The sun's influence is rendered overpoweringly conspicuous because this influence is zonal and varyingly differential, thereby obscuring to a great degree the nearly constant but large effect of inherent earth-radiation.

The earth's desert areas are increasing and the glaciers are retreating not because the sun's influence has seemingly become predominant, but because the earth has, even during known historical time, grown sensibly colder.

At any given time in the history of the earth, an ice age was inaugurated at a given place whenever the snow-fall during the colder months of the year was so great that the snow could not all be melted during the warmer months of the year. But since the earth is continually growing colder the supply of moisture, through evaporation from the water surface of the earth, is continually growing less, so that *finally even the land areas in the polar regions will be completely bare,* and the upper limit of the atmosphere will then practically coincide with the surface of a solidly frozen ocean.

With the modifications, resulting from the

fact that inherent earth-heat has always been and will continue to be a controlling factor in terrestrial phenomena, Manson's hypothesis as to the cause of the ice age may now, perhaps, be said to be demonstrated as a true theory.

Astronomical observations, for absolute parallax of the fixed stars, when made from the surface of a heat-radiating body revolving about another *central* heat-radiating body, are so influenced as to cause the stars to appear to be more distant than they really are. The effect is similar to the case which I have already treated in No. 3935 of the *Astronomische Nachrichten,* issued March 19, 1904.

As will be demonstrated later on, the evidence seems to be almost conclusive that our now reduced sun is the parent body of both the sidereal and the solar systems; the former created by one process simultaneously going on with the creation of the latter by a largely different action of the same forces of nature.

J. M. SCHAEBERLE

ANN ARBOR,
December 25, 1907

PROGRESSUS REI BOTANICÆ

IT may not be amiss to call the attention of botanical workers to the very excellent summary of the present status of paleobotany from the pens of Professors Scott, Zeiller and Laurent which is contained in the first two volumes of the "Progressus Rei Botanicæ" published by the International Association of Botanists under the supervision of Dr. J. P. Lotsy, of Leiden.

There will no longer be any excuse for the lamentable ignorance, too often displayed by botanists, of the striking contributions of paleobotany to the progress of botany.

Professor Scott's article, the first to appear and the most extensive, is entitled "The Present Position of Palæozoic Botany,"[1] and briefly discusses the great plant groups of the Paleozoic, chiefly from the viewpoint of morphology and phylogeny. The quickened interest caused by the recent discoveries of seed-bearing among the fern-like plants and the actual proof of the fern ancestry of at least the cycads among the gymnosperms will serve, no doubt, to save this paper from neglect.

The second article, by Professor Laurent, of Marseilles, is entitled "Les Progrès de la Paléobotanique Angiospermique dans le dernièr décade,"[2] and may be said to treat of the botany of the Cenozoic age, since the floral Cenozoic commences with the dominance of angiosperms in the Mid-Cretaceous. This paper, while it leaves much to be desired, is a thoroughly good, if somewhat philosophical, discussion of angiospermous fossils. It does not go into details as much as would have been desirable and treats of principles rather than the available facts.

The third article, which has just appeared, is entitled "Les Progrès de la Paléobotanique de l'ère des Gymnospermes,"[3] thus roughly corresponding to the Mesozoic age and filling the gap between the contributions of Professors Scott and Laurent. This is a most excellent summary of the recent progress in the realm of Mesozoic botany and no discovery however small seems to have escaped Professor Zeiller's watchful interest. As this treats of the age when cycads and ginkgoes were dominant groups with a world-wide range and when the ancestors of our modern conifers and ferns make their appearance, it is one of very vital interest. Naturally the recent work of Wieland on the Bennettitaceæ receives considerable attention.

A regrettable feature, seemingly part of the plan of the editors, is the absence of citations, except as to authors' names, and the lack of any sort of bibliographies. While this is rendered unnecessary in the first instance by the Paleozoic bibliography compiled by Professor Arber and printed immediately following Professor Scott's paper, it is greatly missed in the other two papers. Paleobotanical workers may be expected to be familiar with the literature, but this is hardly the case with the rank and file of botanical workers to whom these articles are primarily addressed.

It is certainly a cause for congratulation and a distinct sign of progress that the International Association of Botanists recognizes

[1] Erster Band, erstes heft.

[2] Erster Band, zweites heft.

[3] Zweiter Band, erstes heft.

the importance of fossil plants by giving them so prominent a place in the early numbers of their publication.

Edward W. Berry
Johns Hopkins University

THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

The Rockefeller Institute for Medical Research purposes to award for the year 1908–1909 a limited number of scholarships and fellowships for work to be carried on in the laboratories of the institute in New York City, under the following conditions:

The scholarships and fellowships will be granted to assist investigations in experimental pathology, bacteriology, medical zoology, physiology and pharmacology, physiological and pathological chemistry and experimental surgery.

They are open to men and women who are properly qualified to undertake research work in any of the above mentioned subjects and are granted for one year.

The value of these scholarships and fellowships ranges from eight hundred to twelve hundred dollars each.

It is expected that holders of the scholarships and fellowships will devote their entire time to research.

Applications accompanied by proper credentials should be in the hands of the secretary of the Rockefeller Institute not later than April 1, 1908. The announcement of the appointments is made about May 15. The term of service begins preferably on October 1, but, by special arrangement, may be begun at another time.

L. Emmett Holt, M.D.,
Secretary

44 West 55th Street,
New York City

THE UNIVERSITY OF ILLINOIS

OPENING OF THE GRADUATE SCHOOL

The formal opening of the Graduate School of the University of Illinois, which was held on February 4 and 5, marked a significant step in advance, not only for that institution, but also for all the great universities of the middle west that owe their existence to the support of the state. Illinois has maintained a graduate school for some years, but last June the legislature of the state appropriated $50,000 annually for the next two years for the development of the school. This is the first time in the history of American education that the people in their corporate capacity have put themselves on record as definitely in favor of that kind of work the graduate school is doing. This action means much in the development of higher scholarship and research in all those institutions that depend on public money for their income.

The opening address was made by Dr. G. Stanley Hall, president of Clark University, his subject being "The Educational Value and the Danger of University Research." Dr. Hall called attention to the immense sums that the national government has spent for investigations and to the results of greatest practical importance that have come from these investigations. Thus, he said, it is seen that original research pays and that the assertion that state universities should not and can not undertake graduate work is untenable.

The exercises were brought to a conclusion by Professor David Kinley, dean of the Graduate School, who spoke on the subject, "Democracy in Education." In his address Dean Kinley maintained that scholarship of the highest type was not only compatible with the best interests of a democracy, but necessary for its continued existence and advance.

Other speakers were Dean Andrew F. West, of Princeton University, whose subject was "The Personal Qualities of the Graduate Student and their Effect upon his Graduate Studies"; President Rammelkamp, of Illinois College; President McClelland, of Knox College; Robert W. Hunt, consulting engineer, of Chicago, and the Hon. William A. Smith, of the *Engineering Review*. Addresses were also made by Professors Noyes and Greenough, of the university faculty.

In connection with the exercises Dr. W. F. M. Goss was installed as dean of the College of Engineering. Professor Clifford Moore, of Harvard University, also delivered a series

of lectures on "The Last Five Centuries of Western Paganism."

INSTALLATION OF DR. W. F. M. GOSS AS DEAN OF THE COLLEGE OF ENGINEERING

The formal exercises incident to the installation of Dr. W. F. M. Goss as dean of the College of Engineering of the University of Illinois occurred February 5, in connection with the formal opening of the graduate school of the university. The exercises of installation included two sessions and a tour of inspection through the laboratories of the College of Engineering.

The program for the morning session began with a brief address by the president of the university, Dr. Edmund J. James, introducing the chairman of the session, Professor James M. White. Professor Ira O. Baker, who for more than thirty years has been identified with the College of Engineering, described some significant events in the development of the college, giving special emphasis to the work of Stillman W. Robinson, the first professor in the College of Engineering of the University of Illinois, who continued in its service for a period of seven years. Mr. William L. Abbott, president of the board of trustees and a graduate of the College of Engineering, discussed briefly the standing of the technical graduate in the engineering profession. Following this, a formal installation address entitled "The State College of Engineering" was delivered by Dean W. F. M. Goss. This session was made memorable by the presentation of a token of their esteem by his associates in the College of Engineering to Dr. N. Clifford Ricker, a member of the class of 1872, the first professor of architecture, for thirty-five years a member of the instructional staff of the University of Illinois, and for the most of this period dean of the College of Engineering. Letters were read expressing interest in the occasion from Dr. Andrew S. Draper, president of the University of Illinois from 1894 to 1904; from Professor Stillman W. Robinson, the first professor of mechanical engineering, now a resident of Columbus, Ohio, and from Professor J. Burkitt Webb, the first professor of civil engineering, later of Stevens Institute of Technology.

The afternoon session included an address by Mr. Robert W. Hunt, of Chicago, on "The Value of Engineering Research," and an address by Mr. Willard A. Smith, of Chicago, on "The Need of Graduate Courses in Engineering."

During the trip of inspection, which occurred between sessions, the visitors were conducted through the several laboratories. Especial interest was shown in the electric test-car which is a normal interurban car equipped with apparatus for observing and for recording the characteristics of the current absorbed in its operation. This car is operated by the university upon the tracks of the Illinois Traction System. Interest was also shown in the dynamometer car which is the joint property of the Illinois Central Railroad and the university, and which has recently been newly equipped with apparatus for more accurately determining the pull exerted by locomotives and the resistance of trains. The steam engineering laboratory, containing the experimental boiler used by Professor Breckenridge in his elaborate experiments involving the combustion of Illinois fuel, was visited and served as a center of interest for many of the out-of-town guests. The materials testing laboratory, under the direction of Professor Talbot, proved to be of especial interest. The 600,000-pound testing machine, which is installed there, and the work in reinforced concrete, which was seen in progress, proved convincing evidence of the activity of the laboratory in this important field of research.

SCIENTIFIC NOTES AND NEWS

AT a recent meeting of the general council of Glasgow University it was resolved to ask Lord Lister to allow his name to be proposed for the chancellorship, vacant through the death of Lord Kelvin. Lord Lister has replied as follows: "I can not imagine any honor that would have gratified me more had my health permitted to accept it, but I am so enfeebled by illness that there is no prospect of my ever being able to visit Glasgow, and

in my hands the high office of chancellor would be an absolute sinecure."

S. H. Gage, professor of histology and embryology at Cornell University, will retire at the close of the present academic year from teaching to devote himself to research, under the provision of the Carnegie Foundation for the Advancement of Teaching, which provides for an allowance after twenty-five years of service. Dr. James Law, director of the New York Veterinary College, having reached the age of seventy years, will retire with an allowance from the foundation. He will be succeeded by Dr. V. A. Moore, professor of comparative pathology. Professor Gage will be succeeded by Dr. B. F. Kingsbury, now assistant professor of physiology.

Mr. Charles F. Choate, Jr., a Massachusetts lawyer, who graduated from Harvard College in 1888, has been made a regent of the Smithsonian Institution to succeed the Hon. Richard Olney.

Dr. Philip Lenard, professor of physics at Heidelberg, has been elected an honorary member of the Royal Institution of Great Britain.

The following have been elected honorary and foreign members of the Chemical Society: A. E. J. Gautier, Paris; A. Haller, Paris; J. W. Hittorf, Münster; J. A. Le Bel, Paris; H. L. Le Chatelier, Paris; T. W. Richards, Harvard University; and O. Wallach, Göttingen.

The Hon. James Bryce, British Ambassador at Washington, has accepted the appointment to be the next Dodge lecturer on the "Responsibilities of Citizenship," at Yale University. The lectures will be delivered next fall.

Dr. C. B. Robinson, for the past sixteen months assistant curator in the New York Botanical Garden, has sailed for the Philippine Islands, where he has been appointed economic botanist in the Bureau of Science.

Mr. Robert Ridgway has recently left Washington for a six months' ornithological expedition to Costa Rica, in the interests of the U. S. National Museum.

Mr. J. C. Simpson, advanced student, of Emmanuel College, Cambridge, has been appointed to occupy the university table at the laboratory of the Marine Biological Association at Plymouth.

Mr. W. W. Eggleston has been assigned a research scholarship for two months in the New York Botanical Garden to aid him in continuing his work upon North American Thorns.

Dr. Hall-Edwards, of Birmingham, has had his left hand amputated, owing to his having contracted the disease known as "X-ray dermatitis." Dr. Hall-Edwards was the pioneer operator with the Röntgen rays in Great Britain. He was senior radiographer with the Imperial Yeomanry in the South African war, and he is surgeon radiographer to the General Hospital. His right hand is affected, but it is hoped that this will be saved. In spite of his great suffering and the heavy sacrifice, Dr. Hall-Edwards declares his intention of resuming his special work as soon as he is able to do so.

The Special Board for Biology and Geology at Cambridge University reports that the Gordon Wigan income for biology and geology has been applied during 1907 as follows: (*a*) a grant of £50 a year to Dr. D. Sharp for a period of three years, or such part of it during which he holds the curatorship in zoology; (*b*) a grant of £50 for one year to Professor Seward to enable the Botanic Gardens Syndicate to offer greater facilities for plant-breeding experiments; (*c*) a grant of £50 to Professor Hughes to enable Mr. E. A. N. Arber, of Trinity College, to continue his researches into the stratigraphical and geographical distribution of fossil plants.

The Smith's prizes at Cambridge University have been adjudged as follows: W. J. Harrison, B.A., Clare College, for his essay, "Problems in the Wave-motion of Viscous Liquids"; J. E. Littlewood, B.A., Trinity College, for his essay, "On the Asymptotic Behavior of Integral Functions of Zero Order, and Allied Problems"; J. Mercer, B.A., Trinity College, for his essay, "On the Solution of Ordinary Linear Differential Equations having Doubly Periodic Coefficients." The

above names are arranged in alphabetical order. The adjudicators are of opinion that the following essays are deserving of honorable mention, namely, "On Energy Accelerations and Partition of Energy," by C. W. Follett, B.A., Trinity Hall; "On Some Problems in the Theory of Metallic Reflection," by H. R. Hassé, B.A., St. John's College; "The Geometry of Apolar Triads," by W. P. Milne, B.A., Clare College; "Perpetuant Syzygies of the *n*th Kind," by H. T. H. Piaggio, B.A., St. John's College; "The Reflection of Plane Waves of Light at the Surface of a Medium of Special Periodic Character," by C. J. T. Sewell, B.A., Trinity College.

THE eighth lecture in the Harvey Society course will be delivered by Professor Ross G. Harrison, of Yale University, at the New York Academy of Medicine building, on Saturday evening, March 7, at 8:30 P.M. Subject: "Embryonic Transplantation and the Development of the Nervous System."

AT the February meeting of the Philadelphia Section of the American Chemical Society, Professor Leonard P. Kinnicutt, of the Worcester Polytechnic Institute, gave a résumé of the work of the past ten years in sewage treatment, illustrated by slides. He has been asked to give a similar lecture before the Syracuse section of the society at their March meeting.

ON January 30 Professor Koch delivered a lecture, illustrated with lantern slides, on sleeping sickness and the means of combating the disease, in the presence of the German Emperor and Empress.

SIR DYCE DUCKWORTH, lecturer at St. Bartholomew's Hospital and president of the British Section of the Medical Entente, delivered a lecture on February 18 in French at the Faculty of Medicine in Paris, under the presidency of the dean, M. Landouzy, on diathesis —that is to say, the natural predisposition to certain maladies. It is intended that this lecture shall be followed by others, both in London and Paris.

MR. A. WATT, meteorological secretary of the Scottish Meteorological Society, delivered a lecture on the climate of the British Isles to the Royal Scottish Geographical Society, in Edinburgh, on February 19.

PROFESSOR G. HELLMANN, director of the Royal Prussian Meteorological Institute, will deliver a lecture on "The Dawn of Meteorology," at the meeting of the Royal Meteorological Society, on March 11, in the rooms of the Institution of Civil Engineers, London.

A COMMITTEE has been appointed from the senior class of Harvard University to procure a portrait of the late Professor N. S. Shaler to be hung in the living room of the Union.

THE sum of £1,000 has been collected among the patients and friends of the late Dr. W. S. Playfair, and is to be presented to King's College Hospital with a view to erecting a memorial to him at the institution with which he was associated for thirty-five years.

DONATIONS of £1,000 each, in memory of the late Mr. Walter K. Foster, have been promised towards the building fund of the new museum of archeology and of ethnology at Cambridge University by Mrs. Walter K. Foster, Mr. E. Bird Foster, Mr. C. F. Foster and Mrs. E. Rawlings. Mr. Foster, in whose memory this gift has been made, bequeathed to the university in 1891 a valuable collection of prehistoric and Anglo-Saxon antiquities.

OTHNIEL FOSTER NICHOLS, a prominent civil engineer, known for his work on foundations, bridges, tunnels and steel construction, died on February 3, aged sixty-three years, at his home in Brooklyn.

THE death is announced of Mr. George E. Davenport, of Medford, Massachusetts, a student of North American ferns, at the age of seventy-four years.

LIEUTENANT-GENERAL SIR RICHARD STRACHEY, F.R.S., died on February 12, at ninety-one years of age.

THE death is announced of Dr. Johannes Friedrich August von Esmarch, professor of surgery at the University of Kiel and surgeon-general during the Franco-German war, in his eighty-sixth year.

THE deaths are announced of Dr. A. Rümpler, professor of agricultural chemistry in Breslau, and Dr. Rinaldo Ferrini, professor of technological physics at Milan.

Mr. William Kent, of Chicago, Ill., and Kentfield, Cal., has presented a tract of 295 acres of magnificent sequoias in Redwood Canyon, near San Francisco, to the government.

Mrs. Bell Pettigrew, the widow of the late professor of medicine and anatomy at St. Andrews, has given £6,000 for the new museum of natural history of the university.

The large and specially organized herbarium of the Field Museum of Natural History, Chicago, has received through the University of Chicago the complete herbarium of that institution; a herbarium inaugurated and augmented by Professor J. M. Coulter during the past twenty-five or more years of his active botanical researches. With the placing of this herbarium in intimate association with the museum herbarium the university transfers its taxonomic researches to the laboratories of the more fully equipped institution, where the students may receive the associate benefit of conducting their investigations under the supervision of the botanical staff of the museum and of the university. The herbarium thus transferred contains about 50,000 sheets, among which are a large number of types, cotypes and specially studied species.

At a meeting of scientific men held in the Transvaal Museum, Pretoria, on January 13, it was resolved to form an association to be called "The Transvaal Biological Society" with the object of promoting the discussion of scientific problems by biological investigation, to arrange for regular meetings for this purpose, and to publish the proceedings of the meetings. To this society all persons are eligible for membership who are actually engaged in biological investigations, and have published at least one scientific paper, or are working on such. At least six meetings are to be held each year, special meetings to be called by the committee if necessary. Every member is expected to furnish at least one paper each year. The committee for the current year consists of Dr. Theiler, C.M.G., president; Mr. Burtt-Davy, vice-president, and Dr. L. H. Gough, secretary and treasurer.

The German Surgical Society will hold its thirty-seventh annual congress at Berlin from April 21 to 24, under the presidency of Professor Freiherr von Eiselsberg, of Vienna. An exhibition of instruments and apparatus will be held in connection with the congress.

It is announced that an International Positivist Congress will be held at Naples on April 27. The program will consist of two parts, one devoted to the elucidation of positivist principles, the other to their applications to the problems of psychology, ethics, pedagogy, comparative theology, etc. Among those who have promised to read papers are Professor Haeckel, Professor Lombroso, M. Yves Guyot, Professor Villari and Lord Avebury.

A dinner of the Physical Society was held, on February 1, at the Hotel Cecil, London. The chair was taken by the president, Professor J. Perry, F.R.S., and a number of guests and fellows of the society were present, including Sir David Gill, Sir William Ramsay, Sir William White, Sir J. Denison-Pender, Rear Admiral Sir H. B. Jackson, Sir Arthur W. Rücker, Sir Joseph Swan, Colonel R. E. Crompton, C.B., Professor W. G. Adams, F.R.S., Mr. Shelford Bidwell, F.R.S., Dr. C. Chree, F.R.S., Mr. W. Duddell, F.R.S., Professor Carey Foster, F.R.S., Dr. R. T. Glazebrook, F.R.S., Professor Reinold, F.R.S., Mr. J. Swinburne, F.R.S., Professor S. P. Thompson, F.R.S., and Dr. W. Watson, F.R.S.

A new medical society has been formed under the title of Société de Pathologie Exotique, its object being the study of tropical diseases in man and animals, colonial hygiene, naval hygiene, and sanitary measures destined to prevent the extension of epidemics and diseases of animals of exotic origin. The society will meet at the Pasteur Institute. The following officers have been elected: *President,* M. Laveran; *vice-presidents,* MM. Chantemesse and Kermorgant; *General Secretaries,* MM. Marchoux and Mesnil.

At the sitting of the Paris Academy of Sciences on February 17 particulars were given, according to the correspondent of the London *Times,* of investigations by M. Roubaud into the conditions of reproduction of the Tsetse fly, which is the most active instrument in the spread of the sleeping sickness. According to M. Roubaud, this insect seeks

the vicinity of water courses and the shelter of moist plants, and its larvæ perish rapidly if exposed for a few hours to the rays of a tropical sun, even when covered by a thin layer of dry earth. This shows that the Tsetse may be destroyed by cutting down the coverts in which it lays its eggs.

ACCORDING to a law passed by the Danish Rigsdag, the use of the metric system is made compulsory for all government purposes not later than May 4, 1910, the date of adoption prior to the limiting date to be fixed by the king; and two years after this adoption the system is made compulsory throughout the kingdom.

IT is reported that three heavier than air-flying machines, capable of carrying two persons, remaining an hour in the air and traveling at least thirty-six miles an hour, have been contracted for by the United States government. They are to be built by Mr. A. M. Herring, of New York; the Wright Brothers, of Dayton, O., and J. F. Scott, of Chicago.

PROFESSOR A. W. GRABAU, chairman, and Dr. Charles P. Berkey, secretary of the Section of Geology and Geography of the New York Academy of Sciences, have sent out the following notice: "Almost every one appreciates the advantages of general meetings for those interested in similar lines of work or investigation. For geology and mineralogy, the Geological Society of America and Section E of the American Association for the Advancement of Science are accomplishing much in this direction. For various reasons, however, it frequently happens that large sections of the country are poorly represented at the yearly meetings, and the men of those sections are deprived of such advantages for a considerable time. It is also true that, in the larger meetings, the programs are crowded, and therefore opportunities for extended discussions are wanting, and the consideration of local or sectional matters are overshadowed or even forced out entirely by matters of general interest. It would seem that somewhat informal district or sectional meetings might be inaugurated that would meet these conditions. Field trips, such as are arranged occasionally by the geologists of New England, are a partial equivalent, but there is still too little attempted. It is the belief of the members of the Section of Geology and Mineralogy of the New York Academy of Sciences that it would be advisable each year to hold at least two meetings of the geologists and mineralogists of the northeastern section of the United States for the presentation of papers and the cultivation of a closer personal acquaintance. It is not proposed to encourage the formation of any new organization. It seems preferable to enlist the cooperation of the geological departments of colleges and sections of geology and mineralogy in academies of science, surveys and museums, and to depend upon such local units for chief support and guidance. Furthermore, it seems particularly desirable that meetings should be held successively at the different centers of greater activity, so far as they may be also points of easy access; such, for example, as New York, Boston, New Haven, Philadelphia. In accord with these suggestions, believing that they will meet with general approval, the Section of Geology and Mineralogy of the New York Academy of Sciences will arrange for such joint meeting to occupy one or more days early in April. We cordially invite you to attend, and we urge you to forward to the secretary of our section the titles of papers that you wish to present. A program will be printed as soon as these titles can be secured, and this, together with other items of information, will be mailed in advance of the meeting. The date will be the sixth of April."

AN engineer has been despatched to Africa to arrange for the construction of a large dam in the southern part of the German protected territory in Southwest Africa. By this means a reservoir would be created capable of holding about 44,000 million gallons of water. This supply, located about twenty-five miles from Keetmanshoop, would be utilized for the irrigation of the fertile plains below, which are at present incapable of cultivation owing to want of water. Mr. Schmick, the engineer selected for this purpose by Mr. Secretary Dernburg, will likewise visit East Africa, and will survey the courses of several of the rivers in order to ascertain whether they are capable

of being employed in a similar way for irrigation purposes.

UNIVERSITY AND EDUCATIONAL NEWS

We noted last week that by the will of the late Mrs. Frederick Sheldon, Harvard University received $300,000 for the enlargement of the library building or such other purpose as may be preferred, and the residue of the estate for establishing traveling scholarships. The Harvard *Bulletin* states that the residue of the estate will probably exceed half a million dollars, and the total bequest will thus probably amount to more than $800,000. Mrs. Sheldon was the widow of the late Edward Sheldon, of the class of '42.

By the will of the late Mrs. Rylands, the Victoria University, Manchester, will receive the sum of £50,000; Owens College, Manchester, £25,000, and Mansfield College, Oxford, £10,000.

Professor Wilhelm Erb has given the University of Heidelberg a donation of $20,000, one half to be applied for the benefit of students and assistants and their maintenance in hospital when required, the other towards the promotion of scientific research by students.

The council of the senate of Cambridge University have had under their consideration, as we learn from the London *Times*, the desirability of giving the university the power of conferring upon professors who resign their office after having done good service to the university some honor which shall be a public recognition of that service. The title of professor emeritus has been employed for this purpose in various universities; but the existing statutes of this university contain no express provision giving power to confer it. The council think that it would be desirable to obtain that power, to be exercised in cases in which the university may desire to confer the honor. They accordingly recommend that the necessary steps be taken for altering the statutes by the insertion of a paragraph giving the university power, upon the retirement of a professor, either at the date of his retirement or subsequently, to appoint him, on the recommendation of the council of the senate, with the approval of the general board of studies, as a professor emeritus in the subject of the professorship previously held by him. A professor emeritus shall not as such receive any stipend. He shall be subject to no conditions as to duties or residence.

The inauguration of Dr. Edward Dwight Eaton as president of Beloit College took place March 4.

Professor Thos. H. Montgomery, Jr., of the University of Texas, has been appointed professor of zoology at the University of Pennsylvania to fill the vacancy caused by the removal of Professor Edwin G. Conklin to Princeton University. Professor Montgomery was assistant professor at Pennsylvania from 1898 to 1903.

In the faculty of the college of medicine of the University of Wisconsin Dr. Arthur S. Loevenhart, of Johns Hopkins University, has been appointed to the chair of pharmacology and toxicology, and Dr. Charles H. Bunting, of the University of Virginia, to the chair of pathology. Dr. Bunting was graduated from Wisconsin in 1896, and received his medical training at Johns Hopkins. He was subsequently on the faculty of the University of Pennsylvania and on the faculty of the Johns Hopkins University. For the past two years he has been at the University of Virginia. Dr. Loevenhart is also a graduate of the Johns Hopkins Medical School, where, since 1904, he has been associate in physiological chemistry and pharmacology.

Professor F. D. Heald, of the University of Nebraska and botanist of the Nebraska Experiment Station, has been offered the professorship of botany in the University of Texas, at a considerably increased salary. It is not yet known whether or not he will accept the offer.

Dr. Wm. A. Hammond, assistant professor in Cornell University and special lecturer in philosophy at the University of Pennsylvania for the year 1907–8, has been appointed Sage professor of ancient philosophy at Cornell.

Dr. E. A. Darling, has been appointed assistant professor of hygiene at Harvard University.